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ABSTRACT

This "Frame of Reference Study" consists of the fifth section of the final report of the Multi-Disciplinary Graduate Program in Educational Research of the University of Pittsburgh. The term, "frames of reference," is used to mean the context of assumptions, procedures, rules, cognitive models, and conceptions of the nature of evidence that establishes the framework within which an investigator can proceed to detect problems for investigation and formulate strategies for their solution. The study assumed as goals: (1) to devise methods of instruction for presenting alternate frames of reference and their understanding by participants; (2) to conduct a methodology for the study of frames of reference, to explore its feasibility, and to improve it by indepth studies of a number of expert participants. The instructional device developed was a semi-structured, expert-expert interview system with practitioners in the fields of psychology, sociology, psychiatry, and law, which facilitated a growth of communication skills assisting in the transfer of concepts and methodologies from one frame of reference to another and led to an understanding of the coherence of the scholarly approach. Included in the document are (1) an introduction detailing the development of the study, (2) a working paper on the study of frames of reference, (3) an example of staff papers on the work of a participating scholar, (4) a draft paper, "On Doing Empirical Sociology of Knowledge," and (5) conclusions and outlook for the study. (MB)

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THE FRAME OF REFERENCE STUDY

A Component of the Multi-Disciplinary Graduate Program in Educational Research

In the following pages we present the report on the Frame of Reference Study, an investigation and educational pilot project conducted in the context of the Multi-Disciplinary Graduate Program in Educational Research. This project pursued both educational and basic social scientific objectives by addressing itself to the study of scholarly reference frames, as they are manifest in actual investigations. The instructional device of the "expert-expert interview" in the context of a graduate seminar was developed; procedures for instruction in problem detection and solution were established. By focussing on the study of scholarly frames of reference and modes of inquiry, the project makes a contribution to the understanding of multi-disciplinary efforts, and basic processes in the production and communication of knowledge.

The following report presents an overview of the project in the Introduction, followed by a collection of materials including the procedures and instruments used in the investigation, staff papers prepared on the work of the participating scholars that was subject to systematic scrutiny, followed by papers on the theoretical and methodological issues of the investigation.

Contents:

1. Introduction: The Development of the Study
2. The Study of Frames of Reference, a Working Paper
3. An Example of Staff Papers on Work Completed by Participating Scholars: The Work of Arthur Melton

4. Draft "On Doing Empirical Sociology of Knowledge," Mitroff and Holzner

5. Conclusions and Outlook, Holzner

Introduction: The Development of the Frame of Reference Study

In the context of the Multi-Disciplinary Graduate Program in Educational Research it soon became clear that the multi-disciplinary educational setting provided intellectual challenges of a particular kind. Students and professors from a variety of disciplines joined together for discussion of substantive and methodolcgical issues relating to research on education. The multi-disciplinarity of this context illustrated dramatically both the strength and weakness of disciplinary thinking. The coherent cognitive structure of a disciplinary orientation, or even more specifically of that prevailing in a sub-discipline or in a particular group of investigators, is a condition of specialized and detailed knowledge, systematically produced and coherently organized. However, the very strength of systematicity in such orientations also is a source of their limitation, producing professional "tunnel vision," and difficulties in communication.

The Multi-Disciplinary Graduate Program in Educational Research had, from the start, recognized the importance of the problems of multi-disciplinarity and addressed itself to them at first from perspective grounded in the philosophy of science. However, it became quite apparent that the highly abstract perspectives on knowledge and its multiplicity of reference frames, which were to be gained from the philosophical base, were an insufficiently concrete tool both for instructional and organizational

purposes. Organizing experts with different outlooks into a coherent enterprise requires an understanding not only of the epistemological and methodological issues involved, but also of the sociological issues arising from the fact that knowledge is produced in different, and not always necessarily convergent, frames of reference. Of particular importance in this context are the problems of scholarly communication. Several instances occurred in the work of the multi-disciplinary program in which substantive knowledge or methodological devices generated within one discipline found resistance in another, even though they would be highly appropriate to the problems under investigation.

Multi-disciplinary efforts can work in a variety of ways, but for success they require as a minimum a coherent theme, a real problem, and an effective organization. The Multi-Disciplinary Graduate Program in Education at Pittsburgh proceeded at several levels simultaneously. It did pursue the broad theme of research relevant to American education; it further concentrated on several substantive problem areas in which collective efforts would be useful to individual participants, so that knowledge from a variety of perspectives could be brought to bear on the same problem. It also, deliberately and self-consciously, turned its attention to the epistemic and social organizational issues involved in multi-disciplinary research efforts. One major aspect of these enterprises is the frame of reference study.

This study has developed a methodology for the investigation of scholarly frames of reference. By this term is meant the context of often implicit assumptions, cognitive models, procedural rules, and conceptions of the nature of evidence which establishes the framework within

which an investigator can proceed to detect problems for investigation, and formulate strategies for their solution. Naturally, the objective of this study was not simply to show that there are differences in frames of reference. This, in fact, is well known. Lawyers, sociologists, and psychologists -- just to name three examples -- differ sharply in their conception of what constitutes acceptable evidence. The trial lawyer might be reluctant to accept sociological interview results as constituting evidence since they have little similarity to the sworn dispositions or witness accounts in cross examination on which he is accustomed to build conclusive proof. The laboratory psychologist, for example, investigating learning phenomena, is likely to be distrustful of the quality of evidence produced by the participant observer of a group process, and vice versa. It is important to know what the dimensions of difference in frames of reference might be, and in what manner their dynamics function. These matters are important to know both from the point of view of basic social scientific questions, and for the instructional objectives of any multi-disciplinary enterprise. Explicit procedures for the understanding of alternative frames of reference not only aid in professional communication, but also can assist in devising more effective organizational arrangements for cooperation among experts of varying backgrounds.

The frame of reference study therefore set itself several objectives. The instructional objective was to devise methods of advanced instruction suitable for the presentation of alternative frames of reference, and their effective understanding by participants. It relied in this regard on the method of the expert-expert interview before a seminar audience. Instructional sessions of this type require considerable preparation. Experts

selected for participation were asked to identify a body of work they had completed which would become the subject of the interview. In addition, they were supplied with a staff paper to which all participating experts responded. Members of the seminar had read these materials before the session; the interview proceeded on the basis of an interview schedule in a semi-structured format.

The procedure has proven sufficient plausibility that certain merits can be claimed for it. Through the interview procedure it is possible to clarify not only the substantive knowledge gained by an investigator but also his orientation to the problem formulation and methodology. Choices made by the investigator in the course of his research can be revealed as selections from among alternatives, even though the investigator himself may not have given the alternatives systematic consideration. The relationship between basic epistemic and methodological assumptions and actual empirical procedures can be demonstrated in a manner that allows students a comparison of several different approaches. The interview can demonstrate a model for interdisciplinary expert communications. In addition, of course, there are certain "dramaturgical" advantages to the interview format which probably permit the conveyance of more concentrated information than could be absorbed in a one-person lecture of equal duration.

The social scientific objective of this study was to construct a methodology for the study of frames of reference, to explore its feasibility and to improve it by in-depth studies of a small number of select expert participants. The procedure developed allows for the systematic exploration of a body of scholarly work through documentary study and through interviews linking the cognitive orientations of the investigator to institutional

settings and social networks in which he is embedded, as well as to theoretically or "paradigmatically" defined issues of investigation. In fact, one of the main concerns of the exploration dealt with the way in which problems of inquiry are selected and formulated, and how the problem formulation itself may bear on the choice of strategies for solution.

The working group conducting the study was led by Burkart Holzner and Ian Mitroff, and included Evelyn Fisher, Charles Teggatz, Charles Penoi, Leslie Salmon-Cox, Todd Simmonds, and at times Geoffrey Guest. The group and the program of work crystallized from several antecedent concerns. A faculty seminar dealing with the question of the structure of frames of reference in social life, a group with a distinctly social psychological orientation, had been formed by Holzner and Mitroff during the year preceding this study. It was in this context that the objective was formulated to devise a systematic methodology, which appeared as a necessary next step in the progressive clarification of this domain. Ian Mitroff had just completed a major investigation of the orientation systems and modes of inquiry of scientists studying the samples of moon rock brought back by the Appollo missions. Evelyn Fisher, in discussion with Paul Lazarsfeld, recognized the instructional potential of these investigations and provided stimulus for the frame of reference program.

Several steps were necessary in evolving the design of the project. The first phase of intensive work dealt with the clarification of the concept "frame of reference" and resulted in the clarification of the hypothetical structure of reference frames. Clearly, these conceptions were not thought to be final but rather the starting point of the investigation. Several components of frames of reference were identified which were thought

to be capable -- within limits -- of independent variations. These concepts are presented in their early form in the "Working Paper on the Concept Frames of Reference" among the following materials.

On the basis of these reflections a methodology was constructed, giving careful consideration to the kinds of information to be acquired by means of structured instruments, and documentary analysis. Considerable effort was spent in devising a group setting for the "expert-expert" interviews which would foster concentration on a particular level of analysis, which would reveal the working frame of reference of the scholar while avoiding a premature reductionist explanation of it -- such as might occur when an expert participant might become tempted to "psychoanalyze" himself in an amateur fashion. The methodology deliberately emphasized a presentation of the kinds of considerations having a direct bearing on choices made during scholarly work. It also required the collection of information about biographical events and social structural contexts which can, of course, be interpreted from a variety of points of view. The nature of this methodology is described below both in the working paper "Studying Frames of Reference" and in the paper by Mitroff and Holzner "On Doing Empirical Sociology of Knowledge."

Six scholars of distinction agreed to participate in this study; they generously made materials available, wrote memoranda about their own work and prepared for the interview sessions and each participated in two days of interviewing in Pittsburgh. The participating scholars were Robert Glaser, Thomas Fararo, Walter Menninger, Seymour Martin Lipset, Arthur W. Melton, and Thomas Kerr. The group includes two psychologists (Glaser and

Melton), two sociologists (Fararo and Lipset), on psychiatrist (Menninger), and one lawyer (Kerr). The interviews were taped and transcribed; they are being edited and analyzed in the case study format for publication. The selection of participating scholars has provided a very considerable range of orientations, both across disciplines and in the mode of investigative style. For the purpose of the pilot inquiry as well as for the construction of the instructional settings and materials, it was considered desirable to have large differences among the participants, while each of them also addressed themselves to problems in education. These orientations provided for differentiation along the dimensions of applied versus basic science, deductive versus inductive inquiry, survey and historical methods versus laboratory investigations, as well as variations in the standards of proof considered adequate.

The results of this study are highly suggestive for further work. They include a replicable methodology and instructional procedures that can be used by others. Results have been and will be presented at professional meetings as well as in published form.

PROCEDURES

The selection of expert participants.

In order to reach the objective of the program, the expert participants invited must all be persons who have completed investigations resulting in new knowledge in social inquiry. The domain is limited to social investigations in order to simplify the task of comparability; it is not limited to social science as such. For example, it appeared important to include the area of legal investigative techniques. This field certainly falls under the heading of social inquiry but is hardly "social science."

Three disciplines are included: psychology, sociology, and jurisprudence, with the possible extension of the program into economics if resources permit. Within each discipline an attempt is made to have large differences between the approaches of the participants; qualitative, quantitative, primarily theoretical and primarily applied approaches are included.

For example, in the pilot phase of the program in which the procedures were tested and refined two expert participants collaborated: Robert Glaser, an empirical, quantitative psychologist devoted to applied research and development, and Thomas Fararo, a sociologist primarily concerned with the construction of abstract-theoretical, pure sociology through mathematics. Since this study is both didactic and exploratory it is important to maximize significant differences; but it is not important to insist on a systematic sampling scheme. All expert participants are major scholars, presenting recognized alternatives for the conduct of social inquiry. Given this broad scheme individual experts are selected on the basis of far-ranging consultations with informed persons.

The role of the expert participants; the interviews.

The expert participants are requested to prepare a brief memorandum indicating the domain of their work which they propose to make the substance of analysis in this study. This memorandum should include a bibliography and, where materials might not be readily accessible, reprints of all relevant papers. The staff of the frame of reference study and the members of the doctoral seminar will study this material and will prepare a summary paper. This paper is presented to the expert participant for review and circulated among the faculty and students of the Pittsburgh group before his visit. This step makes it unnecessary for the expert to give a lecture presentation of his work. It will already be known in considerable detail.

Two interview sessions then are conducted in Pittsburgh which will cover three major topics. Session I deals with the position of the expert in the network of other scholars and explores his intellectual biography with particular emphasis on the context of the substantive work under discussion. In preparation for this interview the expert participant receives a checklist which should be considered in analogy to an interview schedule. The interview covers the items on the checklist, but not necessarily in the sequence there indicated.

There are no hidden intentions or "unobtrusive measures" built into the interview schedule; the interviews concentrate in a straightforward and public way on experiences, views and attitudes which help to explicate a scholarly frame of reference. Session I will be conducted in a small group of faculty and students, with one of the co-investigators leading the session.

Session II takes place within the doctoral seminar of approximately thirty participants, and consists of two parts. It focuses first upon the problem of investigation chosen by the expert participant and probes the formation of the problem, the evolution of the plan of work and actual research over time. The strategies of investigation and the role of evidence in relation to them are explored.

In the second part of Session II, each expert participant is requested to address the question of the relation between education and social inequality in contemporary America in order to propose an investigation which might help to move the current debate forward. All experts participating in the study receive a staff paper which summarizes the technical debate and the issues involved in it. The points in this paper should form the basis from which the problem area is discussed.

Thus, while the first interviews follow the expert onto his own ground and therefore cover very diverse matters, the study converges in the last interview onto a common arena. It seems reasonable to hope that rather different approaches will be suggested from the different perspectives of participating experts.

Instruments.

The study staff has prepared (or will do so) several instruments used in the work with each expert participant. They have been referred to above and are listed here for convenience:

1. A summary paper of the work to be reviewed which focuses on the problem of investigation identified by the expert himself in his memorandum.
2. The checklist for interview I dealing with the expert participant's position in the social network of scholarship

and his views of epistemological and methodological issues (Appendix I).

3. The checklist for interview II which deals with the review of problem development and strategies of solution (Appendix II).
4. A working paper "Education and Inequality: The Technical Issues" which forms the basis for interview III in which the expert develops his proposals for dealing with the problems of this debate (Appendix III).

The role of the seminar.

The members of the multi-disciplinary doctoral seminar in education participate in the study by attending the seminar and following each interview with a brief period of general discussion. They are active participants, and have influenced the design of this study. Student involvement is important since the pedagogical objectives include the preparation of materials the value of which is being tried in the Pittsburgh seminar.

The role of the study staff.

The two principal investigators, Holzner and Mitroff, together with the coordinator of the multi-disciplinary program, Evelyn Fisher, and the assistants Charles Teggatz and Todd Simonds, form the staff for this study. It is their responsibility to analyze the interviews and prepare from them and the other materials used a set of case study summaries of what has been learned from and about each expert participant, and a systematic analytic monograph which presents what has been learned from the point of view of the sociology of knowledge.

APPENDIX I

Frame of Reference Interview Checklist I: Scholarly Position and Views

1. Briefly, how would you describe your methodological stance?
   -Do other people in your field share this stance?
   -What other approaches do people in your field take which you consider significant?
   -Why do you prefer the approach you take to these others?

2. a) What is the significance of the philosophy of science for your own work, your field, the social sciences?
   -What are the main positive and negative contributions the philosophy of science has made?
   b) What is the significance of the sociology of knowledge for your own work, your field, the social sciences?
   -Are there positive or negative contributions the sociology of knowledge has to offer?

3. One often hears about dichotomies, for example between "hard" and "soft" science, between "objective" and "subjective" approaches and between "pure" and "applied" inquiry. Which of these dichotomies, if any, do you believe to be valid? In what respects?
   -Should the issues these dichotomies characterize be reformulated? How?

4. What is objectivity in social science?

5. What do you consider to be important conditions for the possibility of valid observations?

6. How do you see the relationship between theory, observation, and knowledge application in social science?

7. What general body of work has had the most influence on your own thinking?

   -On your career?

   -Any one person's work in particular?

8. What do you consider to be the domain of systematic social inquiry? Of social science?

   -Where do you place yourself in this domain?

      --Can you describe that position by a "label"?

      --Where would you place yourself "geographically" (at the center, margin, between two components, etc.)?

   -What part of this domain has had the most influence on your own work?

      --Would you describe this influence in terms of specific people? A conceptual framework? A tradition or school of thought?

   -Which other social science comes closest to your own concerns or is of the greatest relevance to you? Which is of the least relevance?

9. Who are the "great" social scientists?

   -Among the contemporaries?

   -Among past social scientists?

   -For all time?

   -Are there "unknown" greats, e.g., under-rated figures?

10. What do you consider as the greatest contribution of the social sciences in the 20th century?

11. What do you consider the most pressing problems facing the social sciences today?

    -Which of these do you feel hamper the growth of the social sciences most significantly?

12. When you think back over your own career to date:

    -What experiences sharpened your own problem focus? For example, graduate school, postdoctoral work, fellowships, research assignments, etc.?

    -What are the most significant contributions you have made?

    -What were the most significant disappointments in your work?

    -Is there anything "not done" which is now irretrievably lost? Why?

    -What are sources of frustration to you?

    -What do you hope to accomplish in the next five years?

13. When you start formulating a problem,

    -Do you start on theoretical grounds (for example, what theoretical structure)?

    -Or from data you have collected?

    -Or actions you have had to take?

    -Or historical circumstances of significance?

    -What other factors enter into your prime considerations?

14. When and under what circumstances have you in the past considered a problem you worked on as solved?

    -Please offer some examples, if any, for solved problems.

    -In what sense were these problems "solved" (empirically, theoretically, through a redefinition of the issue, because of shifts in historical significance, shifts in interest, or other factors)?

APPENDIX II

Checklist for Interview II: Problem of Investigation and Strategies of Solution.

Note: What is to be regarded as the "problem" for the purposes of this part of the interview is formulated by the expert participant's memorandum, his writings, and the staff paper summarizing the work. This interview is designed to explore certain aspects of the social contexts and history of working on this problem.

1. Why and under what circumstances did this problem become significant to you?
   - -What was the first formulation and how did it relate to then existing work?

2. What is the difference between the initial formulation of the problem and the way in which you would formulate the issue now?

3. If possible, could you please give an account of the successive formulations of this problem?
   - -Are there stages in the evolution of your thought about the problem?
   - -What considerations were most salient at each stage?

4. Did the problem remain the "same" throughout the investigation or did its nature change? In what sense?

5. What alternative approaches were considered in the beginning of your investigation and successively throughout it?

   -What alternatives were rejected? Why?

   -What alternatives were pursued? Why?

   -What were anticipated solutions?

   -What did you expect to find in different phases of the investigation?

6. Given the history of this problem, would you identify what you take now to have been critical decisions with respect to problem formulation, anticipated solution, or strategies of solution?

   -On what grounds were these decisions made?

   -Do you now think that these were correct decisions?

7. What is the significance of this problem and its solution?

   -What would change as a consequence of this project in science, in culture, or in society?

8. What evidence supports the proposed solution to the problem?

   -What is the relation between successive formulations of the problem, anticipated solutions, strategies for solution on the one hand and rules of evidence on the other?

   -Did changes in any of the former lead to changes in the latter?

9. Were there any critical data in this work? What brought about the definition of particular pieces of evidence as strategic?

   -What evidence would now lead you to change your mind about the strategic significance of these data?

10. Were there any critical decisions in the progress of the project? What were they?

11. Could you briefly characterize your own epistemology?

APPENDIX III

EDUCATION AND INEQUALITY: THE TECHNICAL ISSUES

by

Todd Simonds

with the cooperation of the Frame of Reference people

Education and Inequality: The Technical Issues

The continuing debate about education and inequality concerns a multi-disciplinary complex of problems, each offering avenues for analysis by social scientists. This paper briefly summarizes several of the issues comprising the debate to provide a problem context for the expert participants in the Frames of Reference Study. Proceeding from the information provided here, each of the experts is asked to devise a research strategy for resolving some issue of the problem. The variety and type of strategies generated and the relationship of these strategies to various frames of reference in the social sciences are important data for this study.

The Inequality Problem in General

The relationship of formal education to social equality is not clearly understood for all its importance for the society at large. The basic assumption from which American educational policy is derived--that equal education is a means for achieving social equality--is regularly challenged by the assertion as fact that education leads to greater social stratification. The fundamental controversy concerns the nature of the relationship between education and social equality, and from that issue several major problems are derived. Assuming a priori the usefulness of education for achieving social equality, the power of education to do this must be measured against other factors that determine the life-chances of people, both those factors that are attributes of the individual, such as family background, and those factors that are manipulable by policy-makers, such as income support and employment laws. Further, the characteristics of equal education, and of good education generally, remain largely undefined: First, what are the educational factors that impact on social equality, and what are the means available to educators for maximizing student performance on those factors? Second, how can educators compensate for student

characteristics that appear to have a negative effect on social outcomes? Finally, the distribution of such education must be measured relative to economic, racial, and geographic groups which obviously poses enormous problems of measurement and data reduction.

The breadth and complexity of the problem is reflected in the numerous volumes that have appeared in recent years addressing the problem of education and inequality. The following discussion sketches the debate as it is defined in five books: James S. Coleman et al., Equality of Educational Opportunity, 1966; Equal Educational Opportunity, 1968, a collection of papers published by the Harvard Educational Review; Frederick Mosteller and Daniel Moynihan, eds., On Equality of Educational Opportunity, 1969; Christopher Jencks, et al., Inequality: A Reassessment of the Effect of Family and Schooling in America, 1972; and Harvard Educational Review, Vol. 43, No. 1, February 1973, "Perspectives on Inequality." The issues addressed by these works are organized according to a framework provided by a work in progress by William W. Cooley and Paul Lohnes entitled Evaluative Inquiry in Education.

Evaluative Inquiry and the Debate

Beneath the political, moral and ad hominem cant that has followed the publication of these works lie two critical problems facing the social scientist: the clarification of values and the clarification of facts. Cooley and Lohnes offer a model for evaluative inquiry that especially treats these two problems, and is particularly useful as a conceptual schema for organizing this debate because it permits a partitioning of the issues. A part of the debate, essentially outside of the realm of social science, concerns the policy interpretations of the information provided by these authors; within the realm of social science, and the focus of this discussion, is the interpretability of the information: Have the authors organized

their information according to the cannons of evaluative inquiry? Cooley and Lohnes discuss at length two critical aspects of evaluative inquiry: valuation and modeling. Around these two aspects much of the debate centers.

Valuation. Valuation is the generation of value-laden operational propositions against which the phenomenon will be measured. Cooley and Lohnes develop their understanding of valuation from a theory of valuation proposed by John Dewey in his 1939 entry to the International Encyclopedia of Unified Science. Dewey contends that value statements do not comprise an absolutely distinctive class of propositions. Rather, any useful value statement is a proposition "stating relations between things as means and other things as consequences, which relations are themselves grounded in empirically ascertained and tested existential relations such as are usually termed those of cause and effect" (p.). Such statements generally assign "a relatively negative value to existing conditions, a comparatively positive value to a prospective set of conditions; and intermediate propositions intended to invoke activities that will bring about a transformation from one state of affairs to another" (p). Further, means and ends are not absolutely distinct, but are "arranged on a continuum such that each condition is a 'means' relative to those conditions that follow it and an ''end' to those conditions that come before it" (C-L, 1.3-5). Values are statements which "direct the flow of behavior at any time" (C-L, 1.3-6). Value statements as goals must be regarded as tentative and testable as means to a subsequent goal. Any piece of evaluative inquiry is obviously limited to one segment of the means-end continuum, but the principle of the goal as tentative and testable yields the principle that "clarification and transformation of

aims or goals of education will be a resultant of, not a prerequisite for, evaluation research" (C-L, 1.3-10).

Modeling. Evaluative research is a cross-breed of experimental and naturalistic correlation research, both in the theoretical knowledge upon which it is built and the procedures employed. Two procedural elements especially give a quasi-experimental shape to evaluation designs: sampling procedures which create intended or unintended differential treatment groups, and the statistical manipulation of variables creating intended or unintended hypothesized causal chains. Cooley and Lohnes propose two principles of statistical modeling for the evaluation researcher: the sample unit and sample structure must reflect the natural reality under examination, and the data reduction techniques must not impose a hidden relationship among variables. Relative to sampling, most educational research has favored the individual as the unit of analysis, arguing that it is the individual, not the aggregate, that learns. Cooley and Lohnes argue that this predisposition is an unfortunate one, first because the school "class" as an aggregate is a particularly meaningful unit in American education, and, more generally, the sample unit ought to be selected on the basis of testable assumptions about the unit in which the critical variable will in fact vary. Too often, they contend, technical sampling brilliance wins a victory over common sense.

Concerning data reduction techniques, Cooley and Lohnes prescribe, first, the use of a small number of linear combinations to represent the multitudinous measurement items in four primary measurement domains (learner characteristics, learning outcomes, contextual dimensions, treatment dimensions), and, secondly, the independent allocation of portion variance to each domain accompanied by the identification of "confounded"

variance as a function of the real interrelatedness of two or more domains. Confounded variance in their model is not a statistical entity; it is rather a statistical measure of the natural complexity of the phenomenon.

Valuation Aspects of the Equality Debate

The valuation portion of the technical debate concerns the transformation of an abstract notion -- equality of educational opportunity -- into an operational proposition and the relationship of that proposition to other socially-valued propositions in a means-end continuum. The continuum that has emerged from the various research programs may be depicted as follows:

1. Equalization of educational resources available to all children yields equalization of achievement outcomes.
2. Equalization of achievement outcomes yields equalization of employment opportunities.
3. Equalization of employment opportunities yields equalization of income.
4. Equalization of income yields equalization of access to socially necessary and/or desirable commodities.

Proposition 4 remains untested within the parameters of the research in question here. Coleman attempted to test Proposition 1, while Jencks attacked each of the first three.

In a retrospective essay in Mosteller-Moynihan, Coleman discusses the thinking of his design team as they attempted to operationalize the concept of "availability of equal educational opportunity," the object of study as mandated by Congress. Five alternatives arose:

a. inequality defined by degree of racial segregation;

b. inequality of resource inputs from the school system;

c. inequality in intangible resources such as teacher morale;

d. inequality of inputs as weighted according to their effectiveness for achievement;

e. inequality of output as prima facie evidence of inequality of opportunity.

The group opted for the fourth, which definition enabled them to rephrase Proposition 1 in operational terms: equality of educational resources is that arrangement of resources which does not prevent equalization of achievement outcomes. They found, of course, that after controlling for family background, the current arrangement of resources as measured by the study did not prevent equal outcomes, that is, could not account for the outcome inequality.

Economists John Kain and Eric Hanushek contend in the same volume that the decision to pursue this line of research distorted the purposes mandated by Congress, and in fact led to the gathering of evidence insufficient to meet the Congressional mandate. They argue that Coleman should have ignored the relationship between resources and outcomes and concentrated on the measurement of inputs available to different groups of students:

> In attempting to do all three (input survey, output survey, process research) the authors of the Report failed to provide convincing answers to the question of whether minority groups are systematically discriminated against in the provision of educational resources (M-M, 118-9).

Coleman replies that:

> by selective attention to one of the definitions of equality of educational opportunity, it implicitly accepts and reinforces that definition. . . . In contrast, the major virtue of the study . . . lay in the fact that it did not accept that definition, and by refusing to do so, has had its major impact in shifting policy attention from its traditional focus on comparison of inputs . . . to a focus on outputs, and the effectiveness of inputs for bringing about changes in output (M-M, 149-50).

In effect, Hanushek and Kain argue for the empirical measurement of one clause in a value proposition as manifest in the nation's schools, while Coleman and his colleagues set out to test the means-end linkage, the reality, of the proposition itself. Cooley and Lohnes have suggested that the latter approach is the ultimate outcome of evaluative inquiry, but the debate continues as to whether the inquiry should be designed to that end or to the narrower problem of measurement.

Edmund S. Gordon (M-M, pp.), summarizes a different stream of criticism related to valuation; that which derives operational definitions of educational opportunity from the fundamentals of the learning process, rather than from the physical and monetary outlays per pupil. He cites four positions:

Melvin Tumin: Equal education consists of "equal pleasure expressed by the teacher with equal vigor at every child's attempt to become something more than he was and equal distress at every failure, with equal rewards for all children.

Ralph Tyler: Equality of educational opportunity is equality of the meaningfulness, stimulation and conditions of learning.

Susan Stodolsky and Gerald Lesser: "Equal opportunity is provided if the school makes maximum use of the distinctive patterns of ability the child possesses."

Kenneth Clark: "The best expressions of (identifiable essential) features (of good education) should be made available to all children alike."

These definitions of educational opportunity appear more sensitive to the dynamics of learning than the resource measures used by Coleman (his findings can be rephrased as "there's is a fairly equal distribution of things that don't count."); further, the discovery of inequalities of this sort would have the effect of redirecting policy from quantitative to qualitative program measures. This is essentially the educationist parallel to the position of economists Kain and Hanushek.

Jencks launches his argument from another direction, attempting to measure the accuracy of the chain of propositions noted above. If equal educational opportunity is proposed as a means to equality of income and status in the adult world, he argues, we should test its efficacy in achieving that. Because he found only "modest relationships between cognitive skill and schooling on the one hand and status and income on the other" (J, 11), he concludes that egalitarian policy should be aimed directly at equalizing income, while school policy should be concerned with the quality of life in the schools.

Coleman and Jencks, then, see as the problem the testing of one or more of the means-end propositions, while the other positions summarized here argue that the first problem is to generate a sounder definition of educational opportunity, then measure the equality of its distribution, leaving the process validation to subsequent research or assumptions. The question remains open as to which perspective, or others not cited, best provides information for decision-making.

Modeling Aspects of the Equality Debate

Many of the articles critical of the Coleman and Jencks reports have concentrated on technical deficiencies of the research programs, concluding that such deficiencies render any conclusions tentative at best. The two critical technical aspects are sampling and statistical modeling, the methods by which the researchers construct the phenomena under scrutiny. The points of contention of each of these aspects for each of the texts are summarized below:

The Coleman Sample. Kain and Hanushek (M-M, 119-23) present the most detailed criticism of the sampling procedure employed by Coleman. First, the apparently large sample (569,000 students) is seriously reduced because they did not obtain input data for students. The school sample is seriously reduced if stratification is desired; for example, there are only four urban Southern schools with a non-white student population between ten and seventy-five percent. Secondly, non-response or faulty response eliminated 51 percent of the high schools. While it is easy to imagine a systematic bias related to resentment of the Congressional motives, such bias was not examined. Similarly, non-response to sensitive questions was unexamined; one third of the northeastern elementary principals, for example, failed to answer questions concerning their attitudes toward the racial composition of their schools. Further, the questionnaire did not seek qualitative assessments of resources or information concerning school organization or the histories of students. Finally, the schools attended by minority students were vastly over-represented. In short, they contend that the Coleman study was based on inadequate information about an unrepresentative group of schools and individuals.

The Jencks Sample. The criticism of the Jencks sample is two-fold and straightforward: "Jancks' analysis . . . eliminates data on black people altogether, and includes only 'native born white non-farm men'" (Edmonds, et al., HER, p. 88); similarly, women and people born since 1936 are excluded from the data used for most of the computations (Rivlin, HER, p. 72). The racial and sexual homogeneity of the sample excludes large numbers of wage earners whose income inequality is especially visible, and the birth cut-off excludes everybody who went through the public schools since the mid-nineteen fifties, a period many educators recognize as the point of emergence of modern educational practice. The Jencks data then cannot answer two critical questions: What does schooling do for groups that have suffered special employment biases? and What do schools as they are now run do to income patterns?

The Coleman Statistical Model. The most comprehensive re-analysis of the Coleman data employing different modeling procedures was produced by Mayeske, et al., and published as Our Nation's Schools, 1969. The authors organized the many Coleman predictors into a small number of scales and replaced the step model with a "commonality" model (C-L, 6-3, 1-2). In the step model employed by Coleman, family background data had been entered first and allowed to account for as much of the variance as it could. The other predictors then parcelled the remainder of accountable variance, leading to the striking conclusion that school characteristics matter very little compared to background characteristics. Mayeske sought to determine which parts of the variance could be uniquely attributed to background and school characteristics, and which part was accounted for by their indivisible co-action. They found the following:

Unique from Background	.11
Unique from School	.05
Commonality of Background and School	.71
Unexplained Variance	.13

(M, p. 139)

Cooley and Lohnes conclude that these findings "dramatize the dilemma that American public schools operate in conjunction with the families and neighborhoods to which they are symbiotically mated . . . one may be pessimistic about the utility of pouring money into schools in poor neighborhoods without operating on the other factors in the marriage" (C-L, 1.4:11-12). This redirection of potential policy decisions stems, they contend, from the greater degree to which the Mayeske statistical model reflects the reality of American education.

The Jencks Statistical Model. Two streams of criticism have been directed against Jencks' statistical model as explained in his Appendix B, a discussion of his path model for analysis. The first stream relates to the policy conclusion Jencks himself draws. Stinchcombe (Science) criticizes him for his emphasis on unexplained variance as an operating factor in determining life success. Against Jencks' reification of "luck" Stinchcombe says that "comparing a real cause in the world with the strongest cause one can imagine, rather than with other causes actually operating, gives an artificially deflated estimate of the importance of the real cause." Thus it seems to him more useful to compare the effects of schooling on income with the effects of other measurable factors excluding chance, or unexplained variance.

The other, more complex, criticisms relate to the accuracy of his path model and to the fact that he apparently tested only one of the innumerable models one could construct from his variables. Stephan Michelson, one of Jencks' collaborators, writes in the HER that Jencks'

statistical demonstration is unrelated to any model of the economy that relates educational attainment and income, while "a number of plausible models suggest that the present economy requires approximately the amount of income inequality we find at present. Jencks needs, at least, to establish a model in which the structure of schooling affects the structure of income, and predicts a value of the school-income correlation so we can determine whether it has this value" (HER, p. 98). The alleged technical deficiencies of the path model -- nonlinearity of some relationships, interaction of variables and missing variables (discussed at length in HER) -- Jencks meets head on and rejects, in part because he tested for linearity and interactions, and in part because the effects of the deficiencies would have been to inflate the apparent values of family and schooling; thus his values for these variables are probably larger than they would be if he listened to his critics (HER, p. 148).

As to other unexamined models, two defenses are raised, one by Jencks in the book and one by one of his critics, Alice Rivlin, in HER. Jencks defends the selection of his single model for analysis as one taken directly from Otis Dudley Duncan's "Ability and Achievement" (Jencks, p. 338) and is grounded, he feels, in both empirical testing by Duncan and in a commonsensical hypothesis about how the variables are related to one another. His goal was not to test the validity of various models; it was to compute accurate values for what he felt to be the best model, testing alternative segments when they suggested themselves. Rivlin's defense derives from her notion of Inequality as an example of "foreasic social science," a new tradition in which "scholars . . . take on the new task of writing briefs for or against particular policy positions" (HER, p. 61). Jencks' self-assumed task was to reject the prominent model

relating schooling to income, and it was thus legitimate for him to concentrate on that model, in effect to attack the propositional chain, rather than trying to recast the variables in new models as in more traditional research.

Conceived as a process of evaluative inquiry, and analyzed according to the principles of such inquiry, the complex debate concerning equality of educational opportunity assumes a somewhat more manageable structure. The issues of valuation -- coming to grips with the meaning and mechanics of equal education -- and modeling -- the development of statistical processes reflecting the reality of schooling -- provide two poles for organizing the debate. The yield of this analysis is a small number of technical assertations and counter assertions, summarized here:

a. The proper design for a study of educational inequality is one which measures the distribution of resources weighted according to their effectiveness for achievement.

versus

The proper design would first measure in detail the distribution of resources, leaving process research as a subsequent undertaking.

b. Educational equality is achieved through equal distribution of physical, monetary and personnel resources.

versus

Educational equality is achieved through attitudinal and process variables applied equally for all children, but differentially according to the characteristics of the child.

c. Education can be a primary road to social equality, and therefore educational policy should be aimed at equal

distribution of inputs with the goal of equalizing outputs.

versus

Educational attainment has only a modest relationship to social equality, and therefore educational policy should be aimed at other goals, such as improving the quality of life for the children and adults in the schools.

d. The samples used by Coleman and Jencks are too unrepresentative to support any conclusions drawn from them.

e. The data reduction techniques employed by Coleman impose a critical bias on his findings; different techniques result in quite different conclusions.

f. The path model employed by Jencks demonstrates the slight effects of education on income equality.

versus

The model is unrelated to any model of the economy which includes education, and is therefore inadequate for drawing conclusions related to the economic effectiveness of schooling,

and

While the path model shows only slight effects for schooling, schooling has the largest effect of any real factor measured, and is a primary target for egalitarian policy.

APPENDIX IV

Working Paper

Frames of Reference

WORKING PAPER ON THE CONCEPT

FRAMES OF REFERENCE

1. PROBLEM

We know that men "make sense" out of the environments they face by selectively and through symbolic constructions defining (interpreting) them as situations. The predispositions and cognitive repertories drawn on in this process of interpretation we will call "frames of reference"; the specific subject-object relation within a defined situation we will call "orientation."

Devising methods for the systematic, empirical study of frames of reference and orientations appears important, because they link presymbolic cognitive predispositions to symbolic repertories, and (through their institutionalization) individual modes of defining situations to those required by roles and institutions (especially in relation to "epistemic communities; sets of roles requiring application of similar epistemic criteria). Thus, the empirical study of frames of reference is likely to be of strategic importance for the sociology of science and of knowledge, as well as for cultural sociology generally. The following steps appear necessary in order to arrive at methods for the study of frames of reference:

1) exposition of the concept itself;
2) exposition of the components of frames of reference as dimensions of expected variation;
3) exploration of the degrees and ways in which frames of reference become observable objects as against observer constructs;

4) exploration of alternative approaches to measurement (not attempted here).

2. THE CONCEPT "FRAME OF REFERENCE"

The interpretation of environments into situations (i.e., the construction of meanings) is always the activity of a subject establishing more or less determinate relations between an "experience" and

a) the subject--thus typically constructing the "experience" as a symbolically representable "object"

b) other objects and their relations.

This activity proceeds on the basis of assigning the subject-object relation ("orientation") a location within a "space" of coordinates, in the most simple case a physical frame of reference locating observer and object in time and space. More complex orientations are embedded in an obviously multi-dimensional, socio-cultural context.

These contexts are describable in analogy to descriptions of space-time locations. In relation to the process of interpretation, however, they function as the taken-for-granted anchorpoints, establishing a frame to which a specific orientation is related or referred which then can be represented as "meaningful." Further, we are here concerned with symbolic interpretations, which involve the selection of some symbol system as appropriate for the representation of the experience. The structure of the preferred symbolism both enables interpretations to be made and constrains their possible variation.

The necessary conditions for the process of intepretation thus include the experience of a subject; a system of taken-for-granted coordinates within which subject-object and object-object relations can be located; and the selection of a system of symbolism as the taken-for-granted medium of cognitive operations. These conditions, together, constitute the "frame of reference." One could describe them in analogy to Kantian "a prioris," of course with the understanding that in distinction from Kant wide variability of these functional a prioris or taken-for-granted contexts of interpretation must be postulated and investigated.

There exists a large literature relevant to this subject, e.g., Dilthey's psychology of world views, Jaspers' more elaborate contributions to the same topic, Pepper's notion of "root metaphors," Thompson's, rather cursory, look at the sociology of "truth strategies," the work of Singer, Churchman, etc. The systematic assessment of the yield of this literature for the present issue requires a paper.

3. COMPONENTS OF FRAMES OF REFERENCE

The following analytic "components" of frames of reference are listed as separate because they appear, in spite of a high degree of interdependence, capable of some independent variations; however, they are to be taken together in any description of a particular frame of reference and the cognitive operations (modes

of inquiry; processes of interpretation) occurring within it or across several frames of reference. The components are

a) preferences for the selection of the experiential base; (preference system);

b) a scheme of categories into which selected information can be tentatively ordered;

c) a "model" of the domain to be inquired into tentatively, but selectively, indicating expected structures and relations;

d) preferred modes of explanation and a repertory of theory;

e) preferred types of reality tests;

f) preferred rules for the mapping of alternative frames of reference.

These will be briefly discussed in turn.

3.1 Preferences for the Selection of the Experiential Base

One function of inquiry is always to reduce uncertainty, or even to produce certainty. There are clearly different bases for the attainment of certainty, here used as a term describing the state of an experiencing subject. They include such things as the certainty of revelation, of mystical states, of public, empirical observations, of self-evident first principles, and the like. The notion is here advanced that the as yet unreflected and in that sense pre-symbolic commitment to a particular mode of experience as a source of certainty becomes the major determinant of the preferences for the selection of experiences to be symbolically represented as data (information). Examples abound

illustrating the fact that inquiring subjects differ, often predictably, in their selective attention to data of various kinds. The predisposition for such selectivity will be called preference system.

However, the underlying commitment to a particular mode of "certainty experience" influences not only preference systems, but also highly symbolically structured, and reflective cognitive operations in the process of interpretation, and reappears in symbolically defined form under the heading of "reality tests" which relate a "proof" as explanation back to a compelling source of certainty.

In relation to the preference system one aspect that needs to be taken into consideration is the source of the original uncertainty which is to be reduced. This may well be of a primarily cognitive or action variety, relating the preferences for new information to a base of specific information needs.

3.2 Categorical Schemes

Data and information, once received, must be organized, related, and stored. A major, and apparently universally appearing, mechanism is the tendency to organize data into and in relation to structured set of concepts--which will be called the "categorical scheme," being a general framework for abstraction. Again, examples are plentiful: it is not difficult to describe categorical schemes for major disciplines (e.g., concepts around "role," "social structure," etc., in sociology) or for particular thinkers. It is important to note that these schemes operate with a high degree of abstraction. The differentiation and structure of such schemes are, of course, variable and ranging probably from simple dichotomies to most complex systems.

3.3 "Models"

Categorical scheme and what is here called "model" are rather closely related, in that the "model" is an image of the relations expected to exist in the domain under inquiry. Often "models" are analogies extended from the familiar and convincing domain of knowledge to an uncertain and unfamiliar one. Pepper's notion of "root metaphors" - metaphoric applications of the familiar to the unknown - which give rise to "world hypotheses" is closely linked to what is here meant by "model."

3.4 Some Comments on the Foregoing and Following Points

This discussion of the components of frames of reference progresses from issues relating to the pre-symbolic experiential base of interpretation (inquiry), through the treatment of matters relating to broadly sensitizing and organizing symbolic processes, to the sharply crystallized symbolic interpretation that is an explanation, or even a "theory," and then back to the experiential base in symbolically defined operations. This far the broadly organizing contexts have been treated; now specific modes of symbolic constructions of interpretation will be mentioned.

3.5 Preferred Modes of Explanation and Theory Construction; Repertories of Theories

Explanations relate a specifically interpreted object, or better an event, by means of a theory to a compelling base of certainty. Their construction involves always, but in varying degrees, symbolically highly disciplined operations which relate the explanation through a compelling mode of "reasoning" to what

is already taken as "known." The matter has been explored in the philosophy of science and a rather wide range of modes of explanation and theory construction have been found. There is every reason to suppose that not only scientists and philospohers explain things and build theories, but that their preferred modes of explanation and theory construction are a subset of those in use in social life generally. It appears that, as yet, there is no systematic attempt to explore this full range of explanatory modes - which certainly would include causal, deductive, genetic, etc., explanations as well as those relying on notions of "agency" rather than cause, on symbolic correspondence or similarity and the like.

The labor of constructing interpretations in an explanatory mode is heavy; clearly there are tendencies to rely on established repertories of theory and even repertories of pre-established specific explanations. Roles and institutions contain as an important component such repertories.

3.6 Reality Tests

Interpretations, constructed in such highly symbolic modes, while always arising out of some preferred experience, have so transformed raw experience into complex constructions that the need to "check" them, refer them back to a base of certainty, arises inevitably. Occasions for such checking, performed in a structured mode, are "reality tests." Some arise inevitably, as an explanation is used in a predictive mode and is either confirmed or not. Others are deliberately sought. Again, the

matter has been explored in the philosophy of science--albeit restrictively. Considerations of empirical tests refer to only one type, to which must be added a wide range such as deductive tests (as distinguished from deductive explanations), tests by referral to authority or "trust," by situation control (or selective sampling) and the like.

One major context for the testing of interpretations, even theories, derives from the anticipated or actual context of their use. In the context of cognitive use knowledge is used, as in institutionalized science, for the organization of existing information and the creation of new knowledge. In the instrumental use knowledge is applied to the calculation of effective means for reaching a goal; in ideological use assertions are applied to legitimate or delegitimate specific claims in the mode of action justification. Or finally the anticipated domain of use may be incorporated into "common knowledge" enlightening the citizenry of a body pol tic.

3.7 The Coherence of Frames of Reference

It appears plausible that variation in one "component" of frames of reference is compatible with only a limited range of variation in others, giving rise to the phenomenon of relatively coherent "cognitive styles"--but this matter cannot be explored here, since the first emphasis might well be on the exploration of the kinds of variation in these components, and the ways in which they can be measured and described.

3.8 Translatability of One Frame of Reference into Another

Only a word on this point: all frames of reference can be, in principle, mapped into each other, but always with some information loss

and other "cost." This point has implications for methods which should be explored.

4. FRAMES OF REFERENCE AS OBJECTS

It is fairly obvious by now that frames of reference are learnable, i.e., can be transformed into symbolically articulated "cultural objects" (Omar Moore's concept). However, a very wide range of variation along this dimension exists from the unreflected, undifferentiated "point of view" of, say, a peasant, to the reflected upon, differentiated, symbolically articulated and maybe even authoritatively codified frame of reference of a scientifically trained agricultural extension agent. This matter might be treated in analogy to Buckley's treatment (following Campbell) of degrees of entitivity of social aggregates; it is obviously a point of major methodological importance, since the empirical study of frames of reference means that the investigator must learn some things about them from his respondents--the modes in which this can be done will differ in large ways depending on the degree of articulation of the frame of reference and the consequent modes of communicability.

FRAMES OF REFERENCE STUDY

Burkart Holzner and Ian Mitroff
Co-Investigators

WORKING PAPER: FRAMES OF REFERENCE IN SOCIAL INQUIRY

MATERIALS FOR PARTICIPANTS OF THE STUDY

December 1973

FRAMES OF REFERENCE STUDY

The "Frames of Reference Study" is a detailed exploration of a few selected perspectives and modes of investigation which have proven significant in certain social inquiries. It is carried out in the context of the Pittsburgh Multi-Disciplinary Doctoral Program in Education and pursues both educational and research goals. A procedure of interviewing experts who have conducted major inquiries is used in order to demonstrate alternatives in problem formation and strategies of investigation, both in principle and in concrete example. A systematic method of discovering and describing the experts' frames of reference is applied. The work explores processes in the production of new knowledge and presents to students tools and principles for dealing with the multi-perspectivity in social inquiry.

This working paper describes background, goals and procedures of the study to participants. The appendices provide the interview checklists, a paper on the current debate concerning education and inequality, and a background working paper on frames of reference.

INTRODUCTION

This investigation, which we call the "Frame of Reference Study" grew out of a convergence of practical needs and theoretical interests. About a year ago Holzner and Mitroff in collaboration with Richard Conviser and several graduate students held informal seminars to work on basic issues in the sociology and psychology of science. These seminars soon concerned themselves with the question of the properties of modes of inquiry and the reference frames of cognitive activity generally. Scientific and scholarly ways of inquiring were analyzed not so much in order to improve scientific methodology but to shed light on the construction of understandings generally, including those involved in folk methods and folk theories.

The same colleagues worked together in the multi-disciplinary doctoral program in education. This enterprise, sponsored by the Learning Research and Development Center at the University of Pittsburgh and funded by the National Institute of Education, brings together advanced doctoral students from a variety of academic disciplines, such as anthropology, economics, history, sociology and others, in order to provide them with opportunities and encouragement to apply their skills to the study of educational problems. The students are working either as fellows or as research assistants on special projects of this program; their departmental dissertation advisors participate as well.

The work of this group includes a continuous research seminar in the deliberations of which there soon arose questions about alternative modes of inquiry and the transferability of concepts and methodologies from one intellectual context into another. Under the initial director of this program, Ian Mitroff, much attention was paid to the nature of

modes of inquiry and their relation to subject matters, and to each other. The current director of the program, Paul Lazarsfeld, focuses even more specifically on the question of the transfer of methodologies and concepts across disciplinary lines, as it is often demanded by the needs of applied research dealing with such multi-faceted issues as those arising in education. Lazarsfeld suggested as a fruitful pedagogical procedure the expert-expert interview in which one scholar conducts a searching interrogation of another investigator, in order to demonstrate to the seminar audience just how a specific kind of scholarly inquiry has proceeded.

Multi-disciplinary education is always difficult and risky. This is especially so when it tries to enable students to acquire special skills for conducting rigorous and serious work in future multi-disciplinary research settings, the structure and tasks of which cannot be exactly foreseen. It therefore seems to be a pedagogical goal of some importance to concentrate on methods for the systematic and thorough understanding of alternative intellectual approaches and modes of inquiry. If such reflection is undertaken in close relation to the investigation of shared objects of inquiry, it should help to produce both more disciplined and skillful observers and procedures for graduate education.

The frame of reference study, designed in this context, therefore has simultaneously pedagogical and research goals. It is, essentially, a procedure for investigating the structure of approaches taken in social inquiry. The general goal is to validate the procedure itself and show that it is appropriate for studying the context of assumptions, often taken for granted, within which problems of scholarly inquiry are formed and strategies for solution developed. A small number of scholars, each

of whom has conducted investigations resulting in new knowledge, are invited to participate and make some aspect of their work the subject of analysis.

RATIONALES

By "Frame of Reference" we mean the structure of assumptions and dispositions which form the context within which inquiries proceed and knowledge is arrived at. These assumptions include epistemologies and methodologies, schemes of categories into which information can be at least tentatively ordered, often a "model" of the domain to be inquired into, preferred modes of explanation and theory, tests of knowledge and anticipations for the significance of the knowledge to be found in theory or use. We expect that a finite typology of reference frames can be constructed and that systematic rules for mapping them can be designed.

Understanding the activities involved in the production of new knowledge seems to us to require a concrete and specific understanding of reference frames. This approach should avoid both the errors of psychological reductionism and of stylized textbook methodology or publication conventions. Psychological reductionism sometimes, in its extreme forms, seems to dissolve knowledge into highly personal productions of personality mechanisms, and the stylized image of science and scholarship tends to obscure the dynamic reality of problem formation and solution. The investigations of the frame of reference study therefore focus on the work of knowledge production and its requirements, and on the social role of the scholar in a very concrete sense. The methodology of interviews conducted in the semi-public setting of interview groups and seminars follows from this rationale.

Two broad aspects of scholarly activity are studied: the location of the scholar in a social network involving colleagues, sponsors, and audiences, and in relation to historical events or trends, and the formation of the problem of inquiry itself and the demands resulting from

it. Both aspects are interrelated, but they become the somewhat separate foci of two phases of the interviews.

Among the pedagogical rationales for this study is the conviction that fruitful work in modern social inquiry requires awareness of alternative modes of inquiry and of the relations of observers and objects in these contexts. Highly abstract representations of the more or less institutionalized frames of reference of the major disciplines do not seem to help much since such matters tend to be depicted rather schematically. Instead it seems useful to devise a pedagogical method which provides procedures for understanding alternative reference frames. This may lead to more sophistication and skill in the transfer of concepts and methods from one context to another. One of the most difficult accomplishments of graduate education is to teach skills in recognizing and forming significant problems. The careful preparation of case studies, resulting from the interviews conducted in this study, should be helpful.

OBJECTIVES

The frame of reference study has both pedagogical and research objectives. While they are analytically separate, they are also clearly interrelated and are here presented as a single list.

1. Clarification of the notion "frame of reference."

Frames of reference vary in their structure and in the degree to which they are articulated by their users. Some have achieved a high degree of self-consciousness and codification and others have not. One objective then is to identify and present the components of working frames of reference used in social inquiry. (Some first steps in that direction have been taken through a working paper and the interview checklists presented in the appendix.)

2. Demonstration of a method for discovering a scholarly frame of reference.

The procedures developed for this study are used for this purpose and are subjected to tests of pedagogical and empirical adequacy.

3. Orientation of students to differently structured frames of reference from varying disciplinary contexts.

This, in a sense is the grossest objective and here the danger exists that it may be misconstrued and misused. It is our intent in interviewing any one expert to develop an object "frame of reference" which can be compared and contrasted to the other objects presented in the other interviews. No attempt will be made to derive the frame of reference of one of the participating disciplines as such; every effort must be made to avoid the impression among students that we have

"encapsulated" the discipline from which our expert participants come. The interviews are to present orientations to special aspects of scholarly practice.

4. Exploration of the dynamics of knowledge production.

Through the rather detailed and close-up review of instances of social inquiry a very fine grained picture can be presented of the dynamics of knowledge production. It is hoped that a contribution can be made to such questions as the forming of paradigms in scholarship and of epistemic communities supporting them. This objective is broad and exploratory but of importance nevertheless.

5. Production of instructional materials.

The interviews and their analyses will become the subject matter for instructional materials hopefully to be used in settings similar to the multi-disciplinary doctoral program within which this study is conducted.

An Example of Staff Papers on Work Completed by Participating Scholars:

The Work of Arthur Melton

by

Evelyn M. Fisher

Arthur Melton, the expert from the discipline of experimental psychology whom we have invited to participate in our Frames of Reference study, has designated the papers that he wishes us to focus upon in our interviews. In addition to these papers, I have included an earlier review paper entitled "Learning" which was published in 1950 in the Annual Review of Psychology and excerpts from two books which Melton edited, Categories of Human Learning, 1964 and Coding Processes in Human Memory, 1972.

I have extended the review of Melton's work beyond the papers designated by our visitor for two reasons. First, by doing so it is possible to use Melton's own reviews of the field and his discussions of conference papers to acquaint those of us who are relatively unfamiliar with the research in this area with the major concerns and shifts in focus over time. Secondly, it exposes us, however inadequately, to the important role that Melton has played in organizing the research of his professional reference group and thereby possibly influencing a systematic attention to the controversies that need to be resolved or the gaps in knowledge that he feels must be filled for general theory construction.

In large measure, I have used Melton's own words and therefore the entire paper may be considered as a series of quotations. I, however, take full responsibility for the selection of these and in doing so for distortion or significant omissions.

In a paper entitled "Learning" published in the Annual Review of Psychology, in 1950, Melton organized a review of the experimental literature in order to point out the controversies that existed and possible directions that research or theory-development might take to lead to a resolution of these controversies. Melton suggested that although there were many contempory theories of learning, the attempt that Hilgard had made to dichotomize them into stimulus-response theories and "field" theories (Hilgard, Theories of Learning, 1948)provided a feasible framework. Furthermore, mapping the research literature in this manner leads to a recognition of the sources or the substance of the controversies.

The types of experiments undertaken by S-R and field theorists and the types of problem situations are not noticeably different. Melton contends that "the basic difference is the nature of the constructs or intervening variables, chiefly the constructs, which are employed in the interpretation of the observed stimulation-organism-behavior relationships."

The field theorists assume that the person through relatively autonomous selection and elaborative operations establishes a field map of the environment and associates different environmental events into cognitions, insights, hypotheses or cognitive maps. These cognitive relationships occur through togetherness in time, contiguity, plus the organizing property of mind. The comprehensiveness of the cognitive map will determine whether the learning is utilized in general form which favors transfer of learning or in a limited "habitual" way which does not favor transfer. Field theorists have

a pluralistic theory of learning and consider the type of learning studied by S-R theorists to be but one type.

For S-R theorists, learning always involves the establishment of connections between stimulating conditions and responses or acts, where the reaction potential is equal to habit strength times the drive strength and the effective reaction potential of a response is determined by the algebraic summation of the reaction potential and inhibitory influences. The effects of learning spread according to principles of stimulus generalization, which assert that the habit strength or inhibitory strength attached to a particular stimulus will generalize to similar stimuli, the amount of generalization being of a function of the degree of similiarity. The S-R theorists have attempted to derive all higher processes from principles which are necessary and sufficient for presumably simpler forms of learning.

Having reviewed these dichotomous approaches to a theory of learning, Melton suggested various controversies to be resolved:

(1) Whether reinforcement is not merely influential but a necessary condition for learning. The major issue here is whether latent learning occurs, that is, whether the organism can learn through processes of perceptual or cognitive organization which depend only on temporal contiguity, in the absence of a temporarily contiguous reinforcing state of affairs. S-R theorists have argued that apparent learning without reinforcement can be explained on the basis of secondary reinforcement and secondary motivation.

(2) Continuity versus noncontinuity in learning. Does the learning process involve a continuous modification of S-R relationships or is it properly described as a discontinuous sequence of organizing acts?

(3) Place Learning versus Response Learning. Is the learning process a specific stimulus-response relationships (response learning) or a cognitive organization or field expectancy (a place or directional disposition).

(4) Transfer of Learning.
The major concern here is to explain the utilization or failure of utilization of previous learning under conditions which differ in some respects from the conditions extant during the original learning.

(5) Retention and Forgetting.
The major advances in this area had not at this time been incorporated into the learning theories of either the S-R or field theorists. Some research had indicated a need for a preservation or consolidation hypotheses in the interpretation of retention and forgetting; other research had focussed in proactive and retroactive inhibitions as the dominant factors.

In 1962, in an address to the Psychology Section of the AAAS, Melton discussed developments that had focussed attention on memory. Learning theorists had revived their interest in the appropriate assumptions to be made about the characteristics of the memory traces that are the products of experiences and repetitions of experience. Several findings of the last few years had focussed attention on the interaction of memory traces during learning as well as interactions at the time of retrieval or utilization in recognition, recall or transfer. An increase in theorising and research on immediate and short-term memory had also directed attention to the need for a general theory of memory.

Learning, which may be defined as the modification of behavior as a function of experience, operationally deals with the question of whether (and, if so, how much) there has been a change in behavior from Trial n to Trial n + 1. It must therefore encompass three processes: trace formation, trace storage and trace utilization-

as well as other processes such as those unique to the several varieties of selective learning and problem solving. Melton suggests that advantages will accrue by considering a general theory of memory to be only a portion of a theory of learning. A theory of memory will be concerned with the storage and retrieval of the residues of demonstrable instances of association formation. A theory of memory will be restricted to a concern for post-perceptual traces, i.e., memory traces, and not with pre-perceptual traces, i.e., stimulus traces. Although stimuli may affect the sensorium for a brief time unless they get "hooked-up", associated or encoded with central or peripheral response components, they do not become a part of a memory-trace system.

Given this restriction to storage and retrieval of traces, the principal issues in a theory of memory are:

(1) Should memory traces be given the characteristic of autonomous decay over time, or should associations, once established, be considered permanent?

(2) Does the memory trace become enhanced by autonomous consolidation through reverberation or preservation? Does it require this in order to become a stable structural memory trace in the central nervous system?

(3) With respect to the morphology of memory, an issue has been whether an all-or-none notion or an incremental notion of association formation (i.e., that the same trace system is activated, reactivated and strengthened) is most accurate or whether both are true.

(4) Are there two kinds of memory storage or only one? It has been contended by those who hold a dual-mechanism view that the Short Term Memory (STM) involves "activity" traces subject to autonomous decay, and has a fixed capacity whereas Long Term Memory (LTM) involves "structural" traces which are irreversible and non-decaying and is infinitely expansible. Those who hold a monistic view ascribe the same properties of LTM to the characteristics of traces of events that occur only once.

Melton considers this last issue - memory as a dichotomy or continuum - significant to the theoretical problems of trace retrieval and utilization. "The conflicting notions with respect to the properties of trace storage and the conflicting notions with respect to the principal determinants of trace retrieval, or failure thereof, converge on the more fundamental issue of the unitary or dual nature of the storage mechanism."

Melton's approach to problem resolution is to examine the alleged differences between STM and LTM in light of recent research on STM. He reviews the experimental data on STM to see whether they are interpretable in terms of the interference factors known to operate in LTM and whether the durability of memory for sub-span and supra-span to-be-remembered units is a continuous function of repetitions. The Peterson & Peterson experiments determined the recallability of single trigrams (eg., XBT) at various intervals from the time of presentation. One second after the trigram was presented, a three digit number occurred and the subject was asked to count backwards by 3 or 4's from that number until they received a cue to recall the trigram. The results revealed a rapid deterioration of performance over time. This did not resolve the question of whether traces from single occurrences are on a continuum with traces from multiple items learned through repetition. A variation of this experiment was conducted by Murdock. Instead of trigrams, Murdock used single common words in one experiment and then word triads (3 unrelated common words as the to-be-remembered unit). The results were that single units showed less forgetting than did trigrams but that some forgetting occured even with

such simple units.

These data suggested that the number of "chunks" in the to-be remembered unit determined the slope of the short-term retention function. Melton considered of even more importance the implication that, other things being equal, the rate of forgetting of a unit presented once is a function of the amount of intra-unit interference and that this intra-unit interference is a function of the number of encoded chunks within the item rather than the number of physical elements, such as letters, or informational units. Melton then conducted a number of experiments to determine the retention curves based on the number of chunks in the to-be-remembered unit, the number of repetitions, and the number of digits that had to be put in between repetitions to work out the repetition effect. The results indicated that events which contain chunks beyond the normal memory span can be brought to the criterion of perfect immediate recall by reducing the number of chunks through repetition and that the structured memory trace established by a single occurrance of an event seemed, by the number of intervening digits that were required to work out the repetition effect, to be extraordinarily persistent. Melton suggests that a single type of storage mechanism is preferable to a dual-storage theory because in such a continuum, frequency of repetition appears to be the important independent variable, "chunking" seems to be the important intervening variable and the slope of the retention curve is the important dependent variable.

In 1964, the contributions of participants in a symposium on "The Psychology of Human Learning" organized by Melton at the University of Michigan were published as a book, Categories of Human Learning.

It was Melton's contention that a taxonomy of human performance requires a taxonomy of human processes, and vice versa. The categories of human learning attended to were restricted to those that had been brought under controlled laboratory observation and did not include perceptual learning, discrimination learning and some forms of attitudinal or emotional learning.

In his section on the development of a taxonomy, Melton says that the noting of the similarities and differences of things and events is the first step in organizing knowledge about nature. These observations are then the basis for classification of things and events and for the formulation of criteria of inclusion and exclusion. A taxonomy reflects both the primitive operational categories and the stages of development of a science. There is a need to limit the generalization of empirical findings to a category or even a subclass of a category, until there is evidence to support a wider generalization. Levels of generality, both intra-category and inter-category, must be achieved either through systematic empirical investigations which bridge boundries within or between postulated categories or by theories which employ hypothetical constructs or intervening variables to reveal the presence of similarities and differences that are more fundamental than those obtained at the observational level. The theories included in the book are still for the most part intra-category theories and as such, are frequently very closely tied to a limited set of experimental operations within the category. Primitive categories have been changed through the refinement of their defining operations and through the identification and differentiation of subclasses of the category.

Major primitive categories have been absorbed into others or major categories have been split into two. Melton predicts that a very complex and radical revision of the primitive categories based on a deeper understanding of the similarities in the processes involved in these various kinds of learning may occur in the near future, as our data and theory permit movement from observables to constructs, from a variety of special theories tied to specific experimental observations to a general theory.

It is clear that psychologists must expect, and are getting, a progressive movement of the taxonomy of human learning processes away from a strictly operational base and toward a theoretical base in which inferred processes become the categories. The theory-based taxonomy will probably supplement rather than supplant the operational taxonomy. The reasons are that the descriptive anchor for the inferred process taxonomy will continue to be the operational taxonomy of learning tasks and the operational taxonomy is likely to continue to serve as an analytic, descriptive tool of the technology of human learning.

In Learning and Individual Differences (Gagne, editor) Melton states, in his chapter on "Individual Differences and Theoretical Process Variables: General Comments on the Conference", that it is necessary that we frame our hypotheses about individual differences in terms of the process constructs of contemporary theories of learning and performance. According to Melton, the most significant development in theorectical and experimental psychology has been acceptance of the need for theoretical statements about processes or mechanisms that intervene between stimuli and responses. The interest in manipulating

and finding individual differences in the hypothesized process will refine the analysis of the process and contribute to a taxonomy of processes.

If there are observable individual differences in performance that can be traced directly to individual differences in a process that is identified in a theory, then the theory gains in predictive power and acceptability; if the process does not vary between individuals, there is probably something wrong with the process construct. Melton distinguishes between a task taxonomy and a process taxonomy. The former has to do with combinations of operationally defined task variables, the latter with inferred processes within the organism. S-R Association and Information Processing are competing theoretical approaches to human learning and performance, both of which use a process language to describe what is going on within the organism between input stimulus and output response. Information Processing theories emphasize "mechanisms" or "acts" that process information as it enters and passes through the nervous system. S-R Association theories seek an explanation of the sequencing of these intervening events and the determination of the output response in terms of relations between antecedent (stimulus) and consequent (response) events that reflect learning and transfer of learning (based on principles that relate transfer to stimulus similarity). These are not incompatible approaches and a convergence may come about.

From the research on recall of three-consonant trigrams, we know that the slope of the short-term memory function is less steep the higher the meaningfulness of the trigrams. The difference between high-meaningful and low-meaningful trigrams can be eliminated by providing the subject with a cue for recoding the trigram into a meaningful unit.

Individuals may be trained to generate their own recoding and it seems probable that the principal factors in verbal learning may be the availability and efficiency of such recoding operations that the subject performs on the sequence of events that is being experienced.

In 1967, Melton comments in "Decision Processes in Retrieval From Memory", (Concepts and the Structure of Memory, Kleinmuntz, ed.) that he is impressed by Peterson's notion that overt responses do not have a one-to-one correspondence with implicit responses to a stimulus, but are rather the outcome of an "editing" process. The individual tests implicit responses and applies a criterion to decide which is the "correct" response. Melton considers various types of tasks in which this seems to occur and he suggests that "our civilization could never have developed if man always automatically said what he thought, never reserved judgement and action until alternatives had been examined, nor experiences uncertainty about the appropriateness of any overt response." Much of the recent research suggests that what were considered unitary processes are made up of sub-processes. Peterson's notion of a deciding process may be a sub-process. Whether it is consistent with the basic tenets of S-R association theory has not been established. "Our response to his model should be directed toward its refinement in the customary give-an-take that scientists engage in when they recognize that an important concept has been identified, but they are uncertain about the details of its operation and application."

Melton goes on to question the deceptive simplicity of Peterson's notion by saying that Peterson has not addressed the problems of (a) what determines whether the stimuli are distinctively coded or the

responses are available in the memory store or (b) how the variations in stimulus coding and response availability affect the outcome of the decision process. Melton suggests other ways of explaining this process which are based on S-R associationist theory. Melton says that his bias is to build complex multidimensional determinations of the implicit response into its eliciting antecedents rather than into a monitoring of the transmission from the implicit into the overt response. Peterson's simple model has "touched a sensitive nerve in an old associationist." Cofer, on the other hand has developed a thesis that stimuli are coded and classified at the time of storage in memory rather than at the time of retrieval. Melton feels that these notions will focus further research and theory on the processes involved in retrieval from memory.

In his article, "The Situation with Respect to the Spacing of Repetitions and Memory" (1970), Melton reviews the papers presented at the Midwestern Psychological Association symposium (May, 1969). These papers deal with the relative effectiveness of massed practice (MP) and distributed practice (DP). Melton suggests that effort had turned away from studying the MP-DP issue on learning other than verbal learning because refinement of theories required that they use the more readily controlled experimentation that was possible in verbal learning. Melton believes that the experimental data that has been amassed suggests that they are on the verge of understanding why and under what conditions repetition improves remembering. In "Meaningfulness and Trigram Recognition" (Martin & Melton) a systematic exploration by means of controlled laboratory experimentation of the role of M (the level of meaningfulness of the verbal unit) in a recognition task is reported. Recognition is

found to be directly related to the M level of the trigrams presented and declines as the number of intervening presentations increases. False recognition is inversely related to M level and increases with the total number of presentations experienced. Martin & Melton point out that the conclusion that the M affects correct recognition in the same way that it affects recall does not necessarily mean that recall and recognition depend on the same underlying processes.

In his preface to Coding Processes in Human Memory, Melton suggests that changes have occurred in the field which are symptoms of a Kuhnian paradigm shift. Melton asserts that that attempts to reconcile the controversies of the decade before 1962 had focussed attention on what was being learned, i.e., stored in memory. A methodological development occurred which allowed questions to be raised and answered about what is stored in memory and about the way what is stored affects retrieval. Pretheoretic conceptions were used as its ban for development of specific theories. "The traditional association theory, which dealt with associative dispositions between input and output events according to a conditioned response analogy, has given way to theories in which the learner is conceived to be an active processor of input events (stimuli) and selector of output events (responses), with the products of learning being conceived as stored perceptual or cognitive events (event traces) and relations between them (associations)." The coding concept - coding, encoding, recoding, decoding, functional stimuli, chunks, subjective units - is the third symptom of a shift. As the term is now being used by psychologists, it has strong mentalistic overtones. However, Melton suggests that there is no necessity that these components

of a coding response to a nominal event be conscious or reportable. There is no need to depend seriously on introspection as a method for identifying the structural properties of a coding response. The experimental methods of the objective psychologists can be employed in further research on this construct. In "The Concept of Coding in Learning - Memory Theory" Melton claims, "Coding is the core concept of what might well be termed a new dynamic structuralism of mental events in which the information processing activities of the learner define the structure of stored traces and these in turn define what is retrievable and what is retrieved, but again with no constraints based on awareness of either the coding process or its product, the code." Melton traces the experimental research that led to this construct - the distinction between the nominal stimulus and the functional stimulus (the stimulus as coded) and the notion of "chunking" in response learning. He reviews the contemporary experimental research which is based on the construct. He concludes by suggesting, "I am confident that we will have gained substantial new knowledge about learning and memory as a consequence of the coding concepts, knowledge and understanding that we would not have gained otherwise."

ON DOING EMPIRICAL SOCIOLOGY OF KNOWLEDGE: SOME METHODOLOGICAL CONSIDERATIONS

by

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Introduction

It is a truism to say that the sociology of knowledge is concerned with the social conditions affecting not only the production of knowledge but the very existence of knowledge itself. (Berger and Luckman, 1966; Holzner, 1968; Habermas, 1973). It is much less a truism to say that one of the central problems of the sociology of knowledge is the role of the various academic and professional disciplines in the construction of knowledge (Churchman, 1971; Mitroff, 1974). Whatever the reasons for their historical establishment and evolution, there is little denying that the disciplines have had an enormous effect. No matter how much lip service is currently given to interdisciplinary and transdisciplinary efforts, the fact remains that the disciplines are still the basic units for the production and organization of knowledge. The influence of the disciplines is such that they are not only a prime factor in the production of knowledge, but they are a prime force in the shaping and molding of the personal attitudes and beliefs of their practitioners (Mitroff, 1974). If the disciplines provide depersonalized, abstract criteria and standards for the assessment of scholarly and professional work, they also provide intense standards for personal association, for example, the formation of life-long friendships and patterns of collaboration. No less important, they provide personal models for the charting and development of careers. Especially the institutionalization of epistemic criteria, i.e., of standards of evidence and of proper communication, is structured in the disciplines.

This is not to imply that the foregoing is necessarily bad. The disciplines have provided and continue to provide necessary and valuable

anchors for the organization of knowledge. However, this is not to say that the picture is all benign either. If, as Russell L. Ackoff (1968) has so aptly put it, "Nature is not organized in the same way that universities are," then the production and organization of knowledge by disciplines is by its very nature artificial and arbitrary (Churchman, 1948, 1953, 1961, 1971). If problems are only abstracted with difficulty from a messy (i.e., a highly interactive) world, then it is neither clear that problems neatly map into the disciplines nor that the disciplines are sufficient to "capture their essence." It is not clear that the essence of problems -- let alone reality -- is captured through a process of divide and conquer by autonomous disciplines (Churchman, 1948, 1971). To put it somewhat differently, if the disciplines allow us literally to "see" certain things that we could not see without them, then it is also the case that they equally prevent us from seeing other things. Disciplines like men have their blind as well as their sightful sides. If the disciplines promote the capacity for seeing, then, as Thorsten Veblen put it with characteristic insight, the disciplines also inculcate "trained incapacity," the ability to ignore and not even see certain phenomena or other "realities" (Foss, 1971, 1973).

This paper constitutes a progress report of a study that the authors have been conducting of various academic and professional "frames of reference." The main purpose of the study is to elicit and to study in depth the means that representatives from various disciplines use to view and to order the social world. While the full report of the study is concerned with both the substantive as well as the methodological results, the present paper focuses almost entirely on the methodological aspects.

The reason is that not only are the substantive results of the study still in process, but also and more importantly, there are comparatively few, if any, papers on a methodology for doing empirical studies in the sociology of knowledge. To be sure the literature is long and full on theoretical studies and papers. However, it is short and lean on empirical results and especially on methodological insights derived from actual empirical studies.

The structure of the paper proceeds on two main parts. The first part consists of an exposition of the methodology for studying the phenomenom of a frame of reference. Major elements of the methodology were evolved on the basis of theoretical work prior to the actual interviews with the participants and have remained fixed over the course of the study. Other elements evolved and grew out of the study itself. Further, some, but not all, of the elements are seen as "necessary" to any empirical inquiry in the sociology of knowledge. Others are merely particular to the present study. Whether any or all of the elements taken collectively of the present methodology are "sufficient" is itself a topic for further inquiry. We would merely note at this point that an over-preoccupation with the question of sufficiency has been one of the main factors retarding the development of the sociology of knowledge. We shall say more about this shortly. Finally, the second part of the paper consists of a series of exhibits (appendices), i.e., the actual interview schedules that we have developed to explicate as well as to study the notion of a "frame of reference."

A word of qualification is in order before proceeding further. It is not the purpose of this paper to review the long history of previous writers whose works bears on the sociology of knowledge and to whom we are deeply indebted. Nor is it the purpose of this paper to offer a

complete or a precise definition of what is meant by concept of a frame of reference. The authors not only doubt that a "complete" and "precise" definition can be given of any concept but that the attempt to do so prior to the conduct of an empirical inquiry may actually be self-defeating. This is not to adopt an extreme empiricist position. If prior to the conduct of an inquiry one is advised not to spend all of one's time seeking a prefect definition, one is also advised to have as clear an idea as possible as to what one is locking for prior to contact with the "it" one is seeking. Otherwise one may not be able to recognize the "it" one is studying upon its presentation.

By the term "frame of reference" the authors mean the underlying structure of cognitive assumptions, personal dispositons and symbol systems which form the context within which inquiries proceed and knowledge is arrived at. These include not only epistemological and methodological assumptions, but also schemas (i.e., categories into which information can be at least tentatively ordered and hence received in the first place. It also includes an inquirer's preferred modes of explanation and theory, including knowledge and reality "tests" by which the inquirer "guarantees" (validates) the knowledge that is produced as a result of his or her preferred way of inquiring. In short, a frame of reference may be characterized by the following:[1]

(1) the set of primitive intellectual elements or notions out of which an inquirer builds an intellectual explanation or model; these primitive elements may be regarded as an inquirer's basic "givens", i.e., what is regarded as a set of basic, unproblematic beginning points;

(2) the set of intellectual operators (i.e., methods) by which an inquirer transforms the basic elements into a set of outcome propositions, i.e., knowledge outcomes;

(3) the set of "reality tests" by which an inquirer guarantees or validates the basic beginning points as well as the outcomes of his or her inquiry process;

(4) an inquirer's cognitive and emotional map of other disciplines; i.e., an inquirer's intellectual and emotional stand towards other disciplines; this is only in part a measure of an inquirer's "breadth of vision;"

(5) an inquirer's relationship to other scholars, i.e., one's place in an intellectual network including who one's "significant others" are; and finally

(6) an inquirer's self-awareness (self-consciousness) of each of the preceding factors including one's self-esteem as a person and as a scholar; this also includes one's awareness of his or her disciplines' history and the degree of articulation of the frame of reference itself. The reader is referred at this point to the first two exhibits in Appendices I and II which contain two of the three sets of interview questions and situations used to elucidate each of the preceding elements of an inquirer's frame of reference.

Some Methodological Considerations

The methodological issues with which we are concerned can be grouped as follows: (1) the issue of self-reflection; (2) the transdisciplinary nature (requirement) of a frame for studying other frames; (3) the nature

of the entry process, i.e., the selection of the participants; (4) the conduct of the interviews, i.e., the processing phase of the study; (5) the analysis of the interviews; and (6) the re-entry phase of the study, i.e., the feedback of results to the interviewees and the public release of the study. We discuss each of these issues in turn.

The Issue of Self-Reflection

The idea of self-reflection or reflexivity is central to virtually all phenomenological theories of knowledge (Habermas, 1973; Hill, 1972; Lobkowicz, 1973; MaCarthy, 1973). As such the concept also plays a central role on the sociology of knowledge (Berger and Luckman, 1966; Holzner, 1968). The issue is the following: First of all, how critically self-conscious must an observer be of himself (i.e., his own inquiry process) before he can critically (i.e., accurately and faithfully) represent as well as study the consciousness (i.e., inquiry processes) of others? Second, under which conditions, if any, can an observer obtain this critical self-consciousness; that is, is such self-consciousness possible? If critical self-consciousness is a prerequisite to the sociology of knowledge, is the sociology of knowledge thereby possible?

While not to belittle the importance of the issues or those who have pursued them it nevertheless seems to the authors that most discussions have lost sight of the original goal. The purpose of raising such issues in the first place was that empirical inquiry might take place in the sociology of knowledge, it was not that reflection would become its own goal endlessly feeding back and turning in upon itself. In the opinion of the authors, if it is the case that the overwhelming majority of sociological inquiries are not reflective enough, it is also the case that

the majority of inquiries in the sociology of knowledge have been too reflective. The result in many cases has been kind of paralysis on the action side. Strangely enough, the sociology of knowledge, which has always been concerned with the relation between theory (or reflection) and practice (or praxis), neglected to develop its own action aspect. To put it somewhat differently, the sociology of knowledge put all its "action" into reflection.

If action and reflection go hand-in-hand and help to illuminate one another, then the purpose of reflection should not be to foster reflection for its own sake, but rather for the purpose of making possible a different kind of empirical inquiry, i.e., reflective empirical inquiry. In the present study, the purpose of reflection is to serve as a constant reminder that in the process of studying the frames of others, the authors must take special care to study and to record as much as possible their own frame, especially as it influences the development of the study itself.

To repeat: the purpose of critical self-consciousness is not to answer all the thorny issues involved in obtaining such consciousness prior to inquiry itself but rather to commit the investigators to a critical study of their own methodological assumptions and decisions over the course of their own inquiry. Paradoxically enough, traditional stances worked against themselves. By dwelling on reflection and further, by asserting if not implicitly assuming its primacy they thereby prevented a certain kind of critical self-study of themselves -- that is, empirical study -- on theoretical grounds.

The Transdisciplinary Requirement

It is far from clear that the conditions that make critical self-consciousness, let alone knowledge itself, possible can be explicated through the use of any single discipline. This is especially the case given the ways that the disciplines are currently constituted. In a word, it is not clear that the central problems of the sociology of knowledge are solely those of sociology. Indeed, the problems of knowledge are not those of any single discipline taken in isolation. They are not even those of all disciplines taken collectively. Instead, it can be argued that an appropriate basis for explicating the problems of knowledge is nothing less than transdisciplinary, i.e., a theory of knowledge that is not the captive of any single discipline but equally well-grounded in all of them. It would seem to be inherently self-contradictory to argue that one can engage in a study of multiple frames of reference, let alone pretend to achieve understanding of them, through the adoption of a frame of reference that is grounded in a single discipline, i.g., sociology. The point is that it would seem that the appropriate frame for studying other frames is nothing less than a theory of knowledge that is transdisciplinary in nature.

It is unfortunately beyond the scope of this paper to outline a transdisciplinary theory of knowledge, let alone the guiding theory this paper presupposes. Of necessity the reader must be referred to previously published works (Ackoff and Emary, 1972; Churchman, 1948, 1953, 1961, 1971; Holzner 1965, 1967, 1972; Mitroff, 1973, 1974). Clearly, such a theory is a specialized one and proceeds within a special frame or reference from which knowledge is studied as an object; it is not to be thought of as a

grand synthesis of all knowledge.

The Entry Process

In order to maximize the possibility of observing the phenomenon of interest, it was decided to pick a relatively small number of as strongly divergent frames of reference as possible for detailed inspection. To strengthen further the possiblity of observing differences, it was also decided to select strong and articulate representatives or advocates for each of the frames. In addition, since we were primarily interested in the operation of different frames within the purview of social inquiry, it was further decided to confine ourselves to the social sciences broadly conceived. The following are the frames and/or orientation that were selected for observation: (1) a representative from the field of cognitive psychology, (2) a mathematically or formally oriented social scientist, (3) an educational psychologist, (4) a psychoanalyst, (5) a lawyer, (6) a phenomenologically oriented social scientist, (7) a political scientist, and finally if possible, (8) a social systems scientist. These obviously nowhere near exhaust the possibilities that could have been chosen.

Specific individuals were chosen on the basis of the following criteria: (1) that they had each achieved some kind of recognition, if not eminence, in their field as indicated by their position, visibility, prestige, awards, etc.; (2) that they were each an articulate and vociferous spokesperson for a particular point of view either within their field or within the social sciences taken generally; and (3) finally, that they are willing to participate in our study. The later criterion

not only required that an individual be willing to spend at least a day-and-a-half in Pittsburgh (at our expense) but that they do some "home-work" prior to their visit.

Initial contact with each individual selected to be studied was made by telephone. The general purposes of the study were briefly explained during the initial encounter. Namely, it was explained that we were engaged in an in-depth study of different points of view in social science and that we considered the individual an articulate spokesperson for a particular point of view. By our call we not only indicated our desire to interview the individual but we also explained that the interview would consist of three main parts. Part 1 probed for general background influences and beliefs (see Appendix I). Part II probed for general and specific beliefs with reference to a particular scholarly and/or professional problem (experience) that had been of importance in the individual's career (see Appendix II). Finally, Part III consisted of the individual's response to a "common object problem," a summary position paper on the Coleman-Jencks debate in the field of education.[2]

It was made explicit that we would send all three parts of the interview schedules to the individuals for their detailed inspection prior to the interview. This was done for two reasons. One, we wanted the individuals to have the opportunity to think about their responses prior to the interviews themselves. And two, we wanted the individuals to have the opportunity to see for themselves what was involved prior to their visit, i.e., prior to their final commitment or consent to being interviewed. In short, we wanted to indicate that we had no "hidden agenda" in mind.

The Process Phase -- The Conduct of the Interviews

As a general rule, the interviews were divided into two distinct phases, a private and a public phase. The first phase, which consisted of the questions contained in Appendix I, was conducted in a relatively "private" setting consisting of anywhere from two to five people. The second phase, which consisted of the questions contained in Appendix II. plus the interviewee's responses to the Coleman-Jencks controversy was conducted as part of an on-going multidisciplinary seminar of twenty or so people. This constituted the public phase of the interview process.

The interviews were divided into a public and a private phase for a variety of reasons. For one, the private phase was not only designed to provide an environment that would, as much as possible, put the interviewees at ease and hence allow us to raise and explore personal issues, but they were also designed to allow for a good amount of give and take between the interviewers and interviewee. The model for these sessions was consciously borrowed from the 18th century. We are referring to the widely known and institutionalized custom process of the frequent exchange of letters between scholars. Although private in the sense of their being addressed to a particular individual, the letters of the 18th century were public in the sense that they addressed themselves to issues that were of concern to all and were written with the knowledge, often conscious as well as deliberate, that they would eventually be made available to general public anyway. Indeed, it is well known that one of the best ways to disseminate a message widely is to initiate it in private to a select few.

The 18th century model was important and conscious in the development of the study in another sense. From the very beginning the interview sessions were conceived of as exchanges between peers, as mutual discussions in the broadest possible sense of the term. The underlying model for our study was truly that of experts interviewing experts.[3] It was not the model of the all-too-typical social science research situation wherein a person of supposedly superior knowledge, the expert, interviews a person of supposedly lesser knowledge, the subject or the interviewee. This point can not be put strongly enough. Although the present study had an explicit interview guide and in this sense the authors were the "experts," to the extent that our "subjects" probed us and that we learned from them as "experts" in their fields, we were as much the subjects of our own study. The point is that whenever sophisticated parties come together, both are equally subject and experimenter and ought to be conceived of as such. It ought to be noted in this regard that if the subjects prepared for our interviews by reviewing our interview questions, then the authors also prepared for the interviews by formally reviewing the work of the interviewees. Indeed, position papers were formally prepared and were required reading prior to the private interview sessions.

If the purpose of the private interview sessions was to draw out the general professional beliefs of the interviewees without reference to specific problems (Appendix I), then the purpose of the public sessions was to draw out the attitudes of the interviewees with regard to a specific problem that had played an important role in their careers (Appendix II). Since the specific problems were unique to each individual,

a position paper summarizing the Coleman-Jencks controversy was prepared and given to each of the interviewees for their reaction. The purpose of this part of the public interview was to draw further the differences between frames by witnessing how they addressed themselves to a common problem. Because of the relevancy of this part of the study, it was conducted before a multidisciplinary seminar in education. By witnessing how scholars from different disciplines handled a common problem, the students in the seminar were given the unique opportunity of witnessing how different disciplinary (background) assumptions affected not only what questions were posed but how they were answered as well.

Exit and Re-entry -- The Analysis and Feedback Phase of the Study

The purpose of this section is not to engage in an extended and detailed discussion of the technical methods by which the interview material will be analyzed. This is particularly the case since the methods for analyzing interview material is well-known. Rather, since the analysis phase of the project is still in its inception, our intent is to lay out some of the various considerations that present themselves.

If social scientists rarely study equals, then they just as rarely present the results of their inquiries to their subjects, and further still, study their subjects' resultant responses. Even rarer is the involvement of subjects in that phase of the research having to do with the analysis of results. The point is that if our subjects are worth interviewing in the first place because they are sophisticated experts of distinction in their own right, then their reactions to the write-up

and analysis of the study is worth collecting in the second place. The use of subjects, especially sophisticated subjects, in the design and analysis phases of social research is still a virtually neglected and woefully underdeveloped aspect of social science.

Since the study of differnt frames of reference is in many respects a study of different disciplinary biases, the authors would be guilty of hypocrisy were we to pretend that we do not have our own biases. It obviously takes a frame to study other frames. Instead of assuming that we or our subjects don't have biases (which we obviously do), the question is, how can we use the fact of our having different points of view to aid us in the analysis of our data? One response is instead of suppressing our biases (which does nothing to eliminate the), we ought to highlight them, to make them as clear and as open as possible. Further, since the interviews constitute an exceedingly rich source of material and as a result can be analyzed from an almost infinite number of different points of view, there is no reason why the authors themselves have to agree on a common interpretation of every piece of the analysis, assuming that we could. Indeed, it begins to become clear that not only ought the interviews not to be analyzed from a single point of view but that the results ought not to be presented in the form of a single report. Instead what occurs is that the results ought to be presented in the form of various position statements each of which represent the analysis of the interview material from a different point of view. That is, multiple analysis ought to form an essential analysis of any study of multiple frames. Finally, the reactions of the interviewees to the multiple analyses ought also to form an essential part of the study.

Concluding Remarks

It is especially fitting to end on a note of self-reflection, for on since we began on a note of self-reflection, and two we argued that self-reflection is central to the sociology of knowledge. We also hope that we have presented enough arguments to show that while the concept of self-reflection is necessary to the sociology of knowledge, it is not sufficient by itself. Or rather, we should say we hoped we have demonstrated that a particular form of self-reflection is not sufficient, i.e., the kind that only dwells upon itself and turns continually inward. Indeed, while everyone of the methodological guides (reflections) we have offered derives from the notion of self-reflection, they are directed for the purpose of going outward, i.e., for the conduct of empircal inquiry. It is high time that the sociology of knolwedge developed its empirical side. We hope this paper constitutes a contribution to this effort.

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FOOTNOTES

1. The reader is referred to previous works for a more complex definition of what we mean by a frame of reference. See Churchman (1971) and Mitroff (1973) and Mitroff and Sagasti (1973).

2. For reasons of space the summary position paper is omitted from the present discussion.

3. We are grateful to Professor Paul F. Lazarsfeld for his emphasis on the pedagogical and intellectual importance of the expert-expert interview, and for his collegial support and cooperation.

APPENDIX I

Frame of Reference Interview Checklist I: Scholarly Position and Views

1. Briefly, how would you describe your methodological stance?
   -Do other people in your field share this stance?
   -What other approaches do people in your field take which you consider significant?
   -Why do you prefer the approach you take to these others?

2. a) What is the significance of the philosophy of science for your own work, your field, the social sciences?
   -What are the main positive and negative contributions the philosophy of science has made?
   b) What is the significance of the sociology of knowledge for your own work, your field, the social sciences?
   -Are there positive or negative contributions the sociology of knowledge has to offer?

3. One often hears about dichotomies, for example between "hard" and "soft" science, between "objective" and "subjective" approaches and between "pure" and "applied" inquiry. Which of these dichotomies, if any, do you believe to be valid? In what respects?
   -Should the issues these dichotomies characterize be reformulated? How?

4. What is objectivity in social science?

5. What do you consider to be important conditions for the possibility of valid observations?

6. How do you see the relationship between theory, observation, and knowledge application in social science?

7. What general body of work has had the most influence on your own thinking?

   -On your career?

   -Any one person's work in particular?

8. What do you consider to be the domain of systematic social inquiry? Of social science?

   -Where do you place yourself in this domain?

      -Can you describe that position by a "label"?

      -Where would you place yourself "geographically" (at the center, margin, between two components, etc.)?

   -What part of this domain has had the most influence on your own work?

      -Would you describe this influence in terms of specific people? A conceptual framework? A tradition or school of thought?

   -Which other social science comes closest to your own concerns or is of the greatest relevance to you? Which is of the least relevance?

9. Who are the "great" social scientists?

   -Among the contemporaries?

   -Among past social scientists?

   -For all time?

   -Are there "unknown" greats, e.g., under-rated figures?

10. What do you consider as the greatest contribution of the social sciences in the 20th century?

11. What do you consider the most pressing problems facing the social sciences today?

   -Which of these do you feel hamper the growth of the social sciences most significantly?

12. When you think back over your own career to date:

   -What experiences sharpened your own problem focus? For example, graduate school, postdoctoral work, fellowships, research assignments, etc.?

   -What are the most significant contributions you have made?

   -What were the most significant disappointments in your work?

   -Is there anything "not done" which is now irretrievably lost? Why?

   -What are sources of frustration to you?

   -What do you hope to accomplish in the next five years?

13. When you start formulating a problem?

-Do you start on theorectical grounds (for example, what theoretical structure)?

-Or from data you have collected?

-Or actions you have had to take?

-Or historical circumstances of significance?

-What other factors enter into your prime considerations?

14. When and under what circumstances have you in the past considered a problem you worked on as solved?

-Please offer some examples, if any, for solved problems.

-In what sense were these problems "solved" (empirically, theoretically, through a redefinition of the issue, because of shifts in historical significance, shifts in interest, or other factors)?

APPENDIX II

Checklist for Interview II: Problem of Investigation and Strategies of Solution.

Note: What is to be regarded as the "problem" for the purposes of this part of the interview is formulated by the expert participant's memorandum, his writings, and the staff paper summarizing the work. This interview is designed to explore certain aspects of the social contexts and history of working on this problem.

1. Why and under what circumstances did this problem become significant to you?
   -What was the first formulation and how did it relate to then existing work?

2. What is the difference between the initial formulation of the problem and the way in which you would formulate the issue now?

3. If possible, could you please give an account of the successive formulations of this problem?
   -Are there stages in the evolution of your thought about the problem?
   -What considerations were most salient at each stage?

4. Did the problem remain the "same" throughout the investigation or did its nature change? In what sense?

5. What alternative approaches were considered in the beginning of your investigation and successively throughout it?

   -What alternatives were rejected? Why?

   -What alternatives were pursued? Why?

   -What were anticipated solutions?

   -What did you expect to find in different phases of the investigation?

6. Given the history of this problem, would you identify what you take now to have been critical decisions with respect to problem formulation, anticipated solution, or strategies of solution?

   -On what grounds were these decisions made?

   -Do you now think that these were correct decisions?

7. What is the significance of this problem and its solution?

   -What would change as a consequence of this project in science, in culture, or in society?

8. What evidence supports the proposed solution to the problem?

   -What is the relation between successive formulations of the problem, anticipated solutions, strategies for solution on the one hand and rules of evidence on the other?

   -Did changes in any of the former lead to changes in the latter?

9. Were there any critical data in this work? What brought about the definition of particular pieces of evidence as strategic?

   -What evidence would now lead you to change your mind about the strategic significance of these data?

10. Were there any critical decisions in the progress of the project? What were they?

11. Could you briefly characterize your own epistemology?

FINAL REPORT FRAME OF REFERENCE STUDY, CONCLUSIONS AND OUTLOOK

This report is written at the termination of the data gathering for the project. The voluminous materials and experiences now available will require considerably more time for in-depth analysis before mature publications can be expected. We are working on these steps now; for that reason the statement of conclusions and outlook at this time must be preliminary and tentative. However, several things can and should be said both about the sociological and instructional yield of the program.

The project was designed to establish frames of reference of social inquirers as objects of investigation. We entered into it with a rather abstract definition and understanding, which the experience of the project has considerably deepened and modified. As we stated in the working paper above (Studying Frames of Reference, p. 5) "by 'frame of reference' we mean the structure of assumptions and dispositions which form the context within which inquiries proceed and knowledge is arrived at. These assumptions include epistemologies and methodologies, schemes and categories into which information can at least tentatively be ordered, often a "model" of the domain to be inquired into, preferred modes of explanation and theory, tests of knowledge and anticipations for the significance of the knowledge to be found in theory or use."

We were quite aware of the complexity hinted at by this conception, and elaborated on it in the conceptual paper ("Working Paper: Frames of Reference"). "The interpretation of environments into situations (i.e., the construction of meanings) is always the activity of a subject establishing more or less determinate relations between 'an experience' and

and (a) the subject -- thus typically constructing the 'experience' as a symbolically representable 'object,' (b) other objects and their object relation ('orientation') a location within a 'space' of coordinates, in the most simple case a physical frame of reference locating observer and object in time and space. More complex orientations are embedded in an obviously multi-dimensional, socio-cultural context.

"These contexts are describable in analogy to descriptions of space-time locations. In relation to the process of interpretation, however, they function as the taken-for-granted anchor points, establishing the frame to which a specific orientation is related or referred which then can be interpreted as 'meaningful.' Further, we are here concerned with symbolic interpretations, which involve the selection of some symbol system as appropriate for the representation of the experience. The structure of the preferred symbolism both enables interpretations to be made and constrains their possible variations. The necessary conditions for the process of interpretation thus include the experience of a subject; a system of taken-for-granted coordinates within which subject-object and object-object relations can be located; and the selection of a system of symbolism as the taken-for-granted medium of cognitive operations. These conditions, together, constitute the'frame of reference.' One could describe them in analogy to Kantian 'a prioris', of course with the understanding that in distinction from Kant wide variability of these functional a prioris or taken-for-granted contexts of interpretation must be postulated and investigated."

It is important for us to emphasize that the formal understanding of frames of reference as outlined above can be maintained after the work we

have done. By frame of reference we do mean the structured context of cognitive "referral points", which become the basis in relation to which an inquirer is enabled to arrive at interpretations of his experience. Specific orientations to a particular problem of inquiry, often needing the establishment of a deliberately chosen set of specific assumptions, concepts, methodologies, and theories, occur within the larger framework of reference.

However, our earlier understanding had been too abstract in the sense that we under-estimated the degree to which frames of reference are anchored in biographical, institutional, and cultural structures; it does not appear that they are easily amenable to change. Powerful motivational forces, indeed, personal and institutional conceptions of identity are involved. As a consequence, for example, it would be unrealistic to consider problem detection and formulation an easily teachable skill. Problems for scientific and scholarly investigation, in the case of every one of the experts who cooperated with us in our study, emerged in the intersect between several different, but always powerful, motivational, institutional and cultural forces.

Further, it is important to point out that problems of investigation are also not easily discarded. The choice of a scientific problem thus emerges as an existential commitment on the part of the investigator as well as a cognitive decision to be justified in methodological and theoretical terms. This point may be obvious to the knowledgeable, but the failure to understand it may very well be one of the roots of the difficulty in teaching the skill of wise choice in problem formulation to graduate students. Perceptions of opportunity, of course, play a major role.

Yet, they are not perceptions of opportunities for ephemeral rewards but -- one is tempted to say -- opportunities for identity, achievement and recogniton in the large sense.

Robert Glaser, for example, detected a gap between scientific knowledge and its application and perceived it as an opportunity for himself. "Taking advantage of" such an opportunity, however, requires total commitment and the investment of enormous personal motivational resources. The existential grounding of problem choices is also dramatically illustrated by Lipset's statement about his early commitment to a socialist youth movement which was "a minority of a minority," the intense internal dynamics of which induced in him an experience of questioning, which imprinted a demonstrable style of searching for politically significant deviant cases in his scholarly effort. One more example might suffice to demonstrate the point; Fararo describes experiencing "a problem of consistency" in his own skills which led him to seek perfection in the mathematical formulation of problems and theories. There is a considerable variety in this matter; but it is a variety in which we believe to be able to detect some order.

The structure of firmly anchored "referral points" within which an investigator formulates his specific and deliberate orientations and programs of effort can be described as the intersect between biographic, institutional, symbolic, and object domains. The formation of a professional identity through biographic experience, including the forming of personal and institutional loyalties and role identification probably is of the greatest significance. The question to be raised here is the nature and scope of the resource investments made; it seems reasonable to expect that with their

variation there will occur a variation in the flexibility or rigidity of frames of reference.

The institutional context in the domain under discussion here refers specifically to the organization of science, or scholarship, and -- most specifically -- to the structure of the epistemic communities and institutions directly involved in the production of knowledge in which the scholar participates. In this regard the nature of epistemic criteria, by which fact is to be differentiated from non-fact, seem to be of the greatest significance. Again, their variation is large between, for example, Melton's preference for experimental laboratory demonstrations, Fararo's for deductive mathematical truths, or the establishments of facts in the public domain as it is being sought by other participants such as Lipset, or the experiential grounding of facts in personal morality as in the case of Menninger. In every instance, however, there is an awareness of institutional requirements with regard to epistemic judgments.

If it may be permissible to speak of institutions that give primacy to such requirements as "epistemic institutions," we may ask questions about the source of the apparently enormous sanctioning capacity they appear to have over scholars. In spite of their variability, epistemic requirements are perceived as absolute by the individual scholar; considerable anxiety and often feelings of guilt and inadequacy are concomitants of this orientation. There is no doubt in our minds that without exception all interviews revealed the operation of astoundingly intense motivational forces operating in this domain. Yet, both the epistemic and the motivational structures vary for each individual; it appears to us now questionable whether a schematically proceeding sociology of science, informed by a highly

generalized philosophy of what science ought to be, can unravel these structures. Again, some order and system can be detected which can guide the search for the source of the sanctioning capacity epistemic institutions exert in scholarship.

The choice of symbol systems for cognitive operations, which is closely correlated to the cognitive style of a scholar, again is a matter of significance. The language of investigation and the rhetoric of presentations are different aspects of this phenomenon, but they are closely interrelated. The language of investigation is much constrained by the structure of the problem at hand (the fourth domain intersecting in reference frames) and by institutionalized requirements. The rhetoric of presentation is more oriented towards the audiences which become targets for communication, varying from the narrow group of peer specialists to larger publics among intellectuals or indeed in the body politic. Surely there must be feedback effects from the rhetoric of presentation to the language of investigation which our analysis will probe.

Finally, the choice of a domain of objects to be investigated imposes constraints on the investigator, demands for learning and the development of skills which are not easily superseded by new choices. In every one of our interviews there was a sense of frustration about the recalcitrance of objects that do not easily yield to the investigator's wishes, but force him to reconsider, pose puzzles some of which may have no solution -- resulting in a sense of profound modesty, sometimes even failure, in persons who otherwise have every reason to pride themselves in accomplishments.

In spite of these complexities, it has become apparent that frames of reference can indeed be constructed as objects of investigation. Clearly it is not the normal mode for an investigator who is problem-oriented to reflect upon his own frame of reference. It requires special occasions, for example, those structured by our methodology. Once they are provided, however, every participating scholar was able to present his frame -- some times unwittingly -- so that it appeared as a virtually palpable object. Frames of reference normally function, in use, as unreflected contexts for one's work. Their coherence and articulation are matters of degree; the coherence, we think, varying with the degree of coherence in the body of work itself, and articulation varying with the requirements for formalization and reflectivity. That is, the greater the latter, the greater the former.

Since giving an account of one's frame of reference is not a normal, routinized and easily performed activity, great care must be taken methodologically in order to avoid certain obvious distortions. Two of these obvious distortions come readily to mind: one goes in the direction of the stylized "textbook" account a scholar might give of his work in which he simply conforms to the rhetoric of presentation customary in his field -- which for good and sociologically understandable reasons suppresses many significant aspects of problem formulation, of choices among cognitive strategies, and of the intensity of motivational investments. The opposite distortion results if the scholar-subject is invited not so much to give a descriptive account of what he does in investigating a problem, but thinks he is invited to render an explantion of it in terms of some theory for such conduct available to him. Many such theories have currency -- psychoanalytic ones, as well as certain simple sociological ones come readily to mind.

It should be obvious that such account giving also would suppress many significant aspects and impose upon the student of frames of reference a great burden of differentiating between the explanations offered by his subjects and those he himself constructs.

In an ultimate sense, this problem has no solution. All accounts of frames of reference are constructed in a social space. It is, however, possible to exclude obviously known distortions and to specify -- as we have done -- the nature of the setting, and the symbolic and epistemic requirements. Our discussions took place in a setting that at once emphasized cordiality and distance, and with considerable emphasis directed attention to educating a group of scholars (the frame of reference group) about the work and the choices at work made by the scholar-subject. This type of setting and focus of the discussion clearly de-emphasized the communication of the purely private -- even though many points were presented and discussed which would hardly enter into a widely public forum. It also de-emphasized giving much time to self-speculation and introspection, in that the body of scholarly work, the production of which was explicated, was more or less consistently in the center of attention. This also was the case with regard to the study of social networks and biographical development, as all these matters were unraveled from the point of view of their relation to the body of research the scholar had produced. The procedure was supplemented by the analysis of published documents, the results of which entered much into our discussions. In fact, two unusual requirements establishing methodological principle were adopted by the frame of reference group, in that its understanding of a scholar's work was formally checked for its adequacy with the scholar himself; and the ultimate accounts

to be given will have to pass a similar test.

Some of the instructional results and procedures of the frame of reference program have already been discussed in the introduction of this report. In spite of all difficulties, modern knowledge-producing institutions require multi-disciplinarity, and therefore it is imperative that at least a significant portion of graduate students become acquainted with the intellectual, organizational, and personal demands of multi-disciplinary settings. Such settings require reflectiveness; that is, an awareness of the multiplicity of frames of reference is not only an intellectual, but also an organizational necessity.

We believe that the frame of reference program has contributed in some measure to the overall effectiveness of the Multi-disciplinary Graduate Program in Educational Research by focussing explicit attention on frames of reference, their structure and on procedures for their adequate comprehension. Its approach recommends itself to us as an instructional procedure to avoid the development of both disciplinary "tunnel vision" characteristic of the single-minded and narrow specialist, as well as of the alternative error -- superficiality and non-commital tolerance that might grant equal validity to any point of view as long as it does not disturb an organizational structure.

Instructional programs focussing on the solving of research problems as such, regardless of disciplinary context, have much to recommend themselves. However, they should be supplemented by an additional explicit attention to the structure and organization of professional reference frames in terms of which the same problem might appear differently.

The instructional device of semi-structured expert-expert interviews with the participation of a seminar for graduate students has proven, we think, effective. It has introduced a deliberate effort at teaching scholarly communication skills which assist in the transfer of concepts and methodologies from one frame of reference into another, and which lead to an appreciation and understanding of the coherence of a scholarly approach. The focus on completed research programs of living scholars to be scrutinized and reviewed in their entirety has introduced an unusual degree of realism into graduate instruction. Case instances of major investigations and the orientations and choices underlying them were made available to the participating students in a format of a sufficiently personalized nature to communicate the seriousness of scholarly endeavor. If anything were to be modified, it would be the length of exposure of a visiting scholar to the student group; this might more profitably be a period of a full week rather than the two days we had available.

The conceptual framework of the frame of reference approach also assisted the program at large. Certain aspects of the terminology used by the frame of reference study group came to be virtually generally used in the program. This appears to have been the case because of the practical utility of these notions for handling the daily requirements of interaction in a multi-disciplinary setting. In this sense the instructional procedures assisted in the development of informal norms for inter-disciplinary and multi-disciplinary contacts. It is therefore necessary to emphasize that the technique of the expert-expert interview appears to have worked especially well because it was embedded in a conceptual framework much of which was readily understandable to all participants. It is gratifying to note that

several groups outside the Multi-disciplinary Graduate Program in Educational Research at the University of Pittsburgh have adopted these techniques. For example, the first year graduate pro-seminar in the sociology department was conducted, in part, using the expert-expert interview device. It appears that the inter-relation between intellectual concerns and instructional procedures characteristic of this program produced an effective opportunity structure for the participating students to further their own learning.